

# Contemporary Psychology

A JOURNAL OF REVIEWS

November 1960

VOLUME V • NUMBER II

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Published by THE AMERICAN PSYCHOLOGICAL ASSOCIATION

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*Contemporary Psychology: A Journal of Reviews* is published monthly. The yearly volume comprises approximately 384 pages. The subscription per year is \$8.00, foreign \$8.50, single number \$1.00.

Published by the American Psychological Association at Prince and Lemon Sts., Lancaster, Pa. and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Lancaster, Pa., and at additional mailing offices.

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# Contemporary Psychology

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VOLUME V

November 1960

NUMBER 11

## Holistic Personality

Robert Ward Leeper and Peter Madison

*Toward Understanding Human Personalities.* New York: Appleton-Century-Crofts, 1959. Pp. xvi + 439. \$5.50.

Reviewed by ARTHUR R. JENSEN

The reviewer identifies the authors in his review. Dr. Jensen himself is Research Associate in the Institute of Personality Assessment and Research at the University of California in Berkeley and also Assistant Professor of Educational Psychology. It was Percival Symonds at Teachers College, Columbia, whose assistant Jensen was for three years, who interested him in the subject of personality. Symonds and Jensen have written a book together on the development of personality, a volume that the Columbia University Press should publish shortly. Between his PhD and his going to Berkeley, Jensen had two years with Eysenck in the Maudsley Laboratory in London. While there he wrote the 1958 chapter on Personality in the Annual Review of Psychology.

THIS book is the fruit of almost a lifetime of thinking about personality. Though Robert Leeper, Chairman of the Department of Psychology at the University of Oregon, is best known for his work in the fields of learning and cognition and for his critiques of Clark Hull and Kurt Lewin, he tells us in the Preface that his major interest has always been in personality. He began thinking about this book as an undergraduate in the 1920s and began writ-

ing it in 1937. He has worked on it steadily for twenty-two years. In the last five years he had as collaborator one of his former students, Peter Madison, a Harvard PhD in clinical psychology, now at Swarthmore College. The book is a complete amalgam of their joint efforts.

The field of personality today is hardly a science in the sense of being in possession of a body of verified laws and theories. It can be called a scientific field only inasmuch as we may regard its subject matter as a realm of natural phenomena which may be subjected to the usual methods of science, that is to say, to systematic description, classification, measurement, analysis, the discovery of functional relationships, and the eventual comprehension of these relationships or laws under a general model or theory.

Headway is made in this endeavor by scrupulous attention to parsimony and precision in the use of language, to operational definition and, in the early stages at least, to simplification and abstraction. All this must proceed in close proximity to empirical realities, to controlled observation and experimentation. The signs of progress in our understanding of personality are becoming apparent, not so much as yet in the form of

substantive knowledge about personality as in the development of methods for studying personality scientifically. Progress is being made, though we are still just at the beginning of a science of personality and still have far to go even in the development of our tools of investigation. But this is the direction we must follow if we are to understand personality in the sense referred to as *Wissenschaft*, that is, in the objective, analytical, nomothetic sense.

In contrast to *Wissenschaft* is another kind of understanding called *Verstehen*, an intuitive, holistic, empathic, appreciative way of viewing and interpreting phenomena in terms of one's own feeling states. It is more in this tradition of *Verstehen* rather than *Wissenschaft* that Leeper and Madison speak of "understanding human personalities." Both the strengths and weaknesses of their work are largely a result of this holistic, idiographic, *Verstehen*-type approach.

In dedicating the book to Köhler and Tolman, the authors acknowledge their indebtedness to the gestalt-field theory. They have written the first introductory textbook in the personality field having a field-theoretical orientation. (Gardner Murphy's *Personality* may come to mind, but it is far more eclectic.) The authors have been influenced also by psychologists such as Adler, Rank, Horney, Sullivan, Carl Rogers, and George Kelly. They also owe much to Freud, but they disapprove of the 'narrowness' of orthodox psychoanalytic theory, with its emphasis on biological drives and the 'negative' aspects of personality.

THE book does not present a new theory of personality nor is it theoretic-



PETER MADISON

cal in any systematic sense. What the authors have attempted and have done admirably well—some might say they have done it too well—is to spread before the reader a vast array of real-life phenomena that are the raw materials of personality research. Much of the book, perhaps half of it, is taken up with colorful, rich, realistically detailed descriptions of various human experiences. In their effort to present a panoramic view of the domain of personality, the authors have drawn upon innumerable, personal anecdotes, case histories, records of psychotherapy, student autobiographies, descriptive anthropology, and works of fiction, in one instance quoting a passage of 10,000 words from a novel by Lillian Smith. On the other hand, we hear nothing at all about such 'colorless' things as measurements, questionnaires, inventories, test scores, correlations, types, traits, factors, or dimensions.

Rather than working toward a science of personality, the authors have merely developed a manner of speaking about personality. It is a manner that will be easy for the layman to grasp in this day when Freud and the unconscious are household terms. The book translates into the language of 'dynamic' and gestalt psychology what are still essentially the layman's ways of thinking about human behavior.

The idea most insistently and pervasively expressed throughout the book

is that personality is like an iceberg, with most of its mass submerged from view. The most important part of personality is what lies below the surface. Behavior itself is interesting only in that it provides clues as to what is going on *underneath* the behavior, or *behind* it, or *inside* the person somewhere. This underlying something, whatever it may be, is not conceived of in physiological terms nor is it linked in any clear or operational way to observable behavior or to events in the environment. The underlying "mechanisms," "processes," "dynamic organizations," and the like are simply a redundant manner of speaking about behavior. For example, the authors explain that a person tires of a particular activity because of "satiation effects," and "satiation effects" are in evidence when a person tires of a particular activity. Obviously nothing of an explanatory nature is achieved by the use of the term *satiation effects*.

"Perceptual processes" carry the greatest burden of explanation. The dogs in the Solomon and Wynne experiment are said to go on avoiding the electric-shock box even when there is no longer any shock because there is no change in their perception of the situation. When the dogs cease jumping, it is because of a change in their perception. The behavioristic, anxiety-reduction interpretations of Solomon and Wynne, Dollard and Miller, and Mowrer are not hinted at. Yet the question is not even raised concerning how the perceptual change comes about.

The authors do not regard all individual differences in human behavior as personality. Personality refers only to "emotionally significant processes." It is how the person perceives and deals with things of emotional significance to him. Personality is largely learned (learning consists of a change in perception); it is also a resultant of the reintegration of past experiences brought about by the forces of the immediate psychological field.

**T**HE critical reader is apt to become confused by the lack of definitional clarity in the authors' manner of speaking about personality. Often the key words outnumber the actual concepts to

which they refer—for example, *processes* and *mechanisms*, *conflict* and *disunity*. We read that "personality processes are perceptual processes." Also "perceptual processes may be motivational processes." "Emotions are motives" and "emotional processes are perceptual processes." These "processes" are never anchored in any way to observables. The closest the authors ever come to doing so is to state that "the development of emotional motives . . . results from the formation and growth of neural systems that the individual originally did not possess" (p. 217). Many of the explanations by analogy are more puzzling than clarifying. ("Perceptual processes can be motivational processes at the same time they are perceptual processes, just as it is true that a person is living in Pennsylvania at the same time he is living in Philadelphia;" "The world topples into war because it resembles a pyramid standing on its point instead of on its base.")

Another manner of thinking about behavior that the book inculcates, perhaps inadvertently through the loose use of language, is the notion that we "use" habits, we "use" motives, emotions, "reintegrative mechanisms," and so on, as if we possessed a store of mechanisms or processes within us that we could call upon in various circumstances.

For what audience is the book in-



ROBERT WARD LEEPER



tended? This question arouses my most serious concern. The authors have expressly addressed themselves primarily to the psychology undergraduate taking his first course in personality. Certainly the book will be easy and interesting for this audience. The style is smooth and very readable. The approach is appropriately didactic for the undergraduate; in every chapter the authors adhere to the rule of first telling the reader what they are going to say, then saying it, and then telling the reader what they have said. Consequently, more advanced students may feel that the book is longer than necessary for its essential contents.

My concern is that I greatly doubt that this book will attract into psychology or into the area of personality those students who have a scientific bent. Yet it is they who are the future hope of psychology. This book does not suggest that personality can be treated as a natural science amenable to rigorous research. Actually, for an introductory text in a field at this stage in its development, the text is probably not sufficiently eclectic. It contains hardly any mention of biological and hereditary aspects of personality, or of the great amount of work that has already been done in the measurement and assessment of personality, or of the statistical and experimental methods that might advance our knowledge of personality. In this respect the exposition stands in marked contrast to the texts of, say, Cattell and Guilford. The names of such outstanding researchers in personality as Cattell and Eysenck are not even mentioned, and Guilford's name appears only in the bibliography. Indeed, the authors eschew any mention of factor-analytic or behavioristic research in personality. Occasionally they even make slighting remarks about experimentation and laboratory investigation as being "colorless and neutral."

On the other hand, this is a book that the researcher in personality may well afford to read, if only to be reminded of the richness and complexity of his domain. Still we know that if we are to make any real progress in a science of personality, we must be content with a degree of simplicity that permits functional analysis, even though we must

seek it in the laboratory. However one may disapprove the authors' depreciation of this point of view, they must nevertheless be commended for achieving their unique purpose of presenting personality in a richly human perspective.

## A Clinical Laboratory?

Gerald R. Pascal

*Behavioral Change in the Clinic: A Systematic Approach.* New York: Grune & Stratton, 1959. Pp. viii + 128. \$4.75.

Reviewed by HARRY G. YAMAGUCHI

*who is Associate Professor of Psychology at Indiana University, where he has been since 1951. He thinks of himself as a Hullian clinical experimentalist and he was collaborating with Clark Hull when the Yale group was working on the quantification of the reaction potential.*

THE scientific method can be applied to the problem of changing human behavior. This is the main theme of the book by Gerald R. Pascal, who is Professor of Psychology, Director of the Clinical Training Program in Psychology, and Director of the Psychological Service Center at the University of Tennessee. His book presents the theory of behavior and the techniques of psychotherapy that he has been teaching for several years and it is written mainly for students of clinical psychology. The terminology of the system as well as the principles of the theory come directly from the experimental laboratory. Thus, in the language of the system, one does not say that a patient goes to a clinic to see a psychotherapist. Instead, one says that a subject goes to a clinical laboratory to see an experimenter. If psychotherapy is done for a fee, should one say that the subject would pay the experimenter? Should subjects pay experimenters?

The basic theory of the system is mostly a blend of Hull, Lewin, and Tolman. It states: *psychologic deficit is a function of the multiplicative relationship between stress, habit, psychophylaxis, and environment.* Psychologic deficit refers to directly observable behavior, not to feelings or attitudes, exhibited in response to specific stimuli in an individual's real-life environment, and it is measured by the extent to which an individual's response deviates from the average response of other persons of similar status in the same culture. Stress is the motivational variable and is produced when an expectancy is frustrated. Two kinds of habits are postulated. Deficit positive habits are attitudes and feelings with behavioral manifestations resulting in psychologic deficit. Defensive habits counteract the effects of deficit positive habits. Psychophylaxis is the resistance to psychologic deficit and is estimated from the individual's behavior history.

The theory has a number of implications for clinical strategy and forms the basis for the system's three main methods of reducing psychological deficit. The "Type I" approach is an environmental manipulation technique aimed at reducing stress. The "Type II" approach is a counterconditioning procedure whereby inefficient defensive habits are extinguished and more efficient defensive habits are reinforced. The "Type III" approach is regarded as being basically an extinction procedure applied to deficit positive habits. This approach is lengthy, requiring several hundred hours, and is complex. After the therapist has structured himself as an accepting and understanding person, he "promotes generalization" to himself from the original stimuli associated with the deficit positive habits. He is nondirective, darkens the room, and has the patient lie on a couch. These procedures induce the patient to react as if the therapist were somebody else—a parent figure, for example—and the therapist maintains the illusion. Since the therapist accepts the patient's deficit positive behavior, the original stress is not produced and extinction is effected. The terminal phase of this approach involves "emotional reintegration" which is accomplished by the therapist's restructuring

turing himself as an accepting and understanding person.

Pascal's system represents a sizable step in bridging the gap between clinical and experimental psychology. Judging from the research it has already stimulated, the system should continue to develop.

**I**N this reviewer's opinion, a major shortcoming of the book is the lack of sufficient detail in the description of the therapeutic procedures. It would be virtually impossible to replicate any of Pascal's 'experiments'—and he himself regards each case as a single experiment—without special information concerning procedure. It is also impossible for the reader to judge whether the stated principles are sufficient to account for the procedures used and the results obtained. Pascal says: "The principles from which the Type III approach is derived stem from the experimental laboratory. These principles are not peculiar to the human organism. It seems fairly reasonable, for instance, to think of the Type III approach applied to a dog." If, for example, a dog has the psychologic deficit of chasing and biting cyclists, a training procedure using the principles of reinforcement, extinction, and generalization might indeed produce a benign change in behavior. But such a procedure would hardly be the same as conducting nondirective interviews with the dog on a couch. The question of whether all of the details of the procedure of the Type III approach can be subsumed under the principles of reinforcement, extinction, and generalization would still remain.

The book is intended to be helpful to the student clinician. If he is bothered by the lack of scientific respectability for his clinical concepts and activities, the book cannot help him much. It is true that Pascal insists on changes in overt behavior as the criterion for evaluating the effects of therapy, and that he does not regard bladder distension as a stimulus since it is not a discernible and definable condition external to the organism. These views are quite respectable by hardheaded standards. Still, at the end of the book, Pascal says: "Our constructs have no scientific status." If a student wants to become

a disciple of Pascal's system, he will have some loyalty problems. In one passage, he will read: "The student needs to hold fast to his systematic position." In another passage, he is advised "not to become 'stuck' with this or any other systematic position in our present state of knowledge." How can the student tell when he is holding fast or when he has become stuck? On the positive side, he will probably be happy to learn that therapy is a matter of applying a few basic learning principles with the proper techniques.

## The Empty Correlation

Franklin Loehr

### *The Power of Prayer on Plants.*

Garden City, N. Y.: Doubleday, 1959. Pp. 144. \$3.50.

Reviewed by RICHARD H. WILLIS

*who is Assistant Professor of Industrial Administration and Psychology at the Carnegie Institute of Technology in Pittsburgh. He is a Wisconsin PhD, interested in attitude scaling and decision processes, and CP supported his suggestion that we see how the Galtonian scientific attitude toward theological intervention looks to the experimental theologian of the present day. Just at present he is Fulbright Lecturer at the Tampere School of Social Sciences in Finland.*

**I**N 1883, Sir Francis Galton opened his essay entitled *Objective Efficacy of Prayer* with the following words:

It is asserted by some, that men possess the faculty of obtaining results over which they have little or no direct personal control, by means of devout and earnest prayer, while others doubt the truth of this assertion. The question regards a matter of fact, that has to be determined by observation and not by authority; and it is one that appears to be a very suitable topic for statistical inquiry.

Now, over 75 years later, a book ap-

pears which attempts to settle this "matter of fact" by reporting an extensive series of experiments testing the hypothesis that prayer can influence the growth of plants. If one accepts the conclusion that Reverend Loehr draws from his experimentation—that prayer can effect plant growth—it follows that his book is a most important scientific document. If one rejects his conclusion, one must still grant that this book merits close attention by psychologists and others whose business it is to consider the evaluation of scientific evidence.

Suppose a new issue of one the psychological journals to which you subscribe arrives in the next mail, and that the first paragraph to catch your eye reads as follows:

Twenty-three cases were run in both the experimental group and the control group. Sixteen cases in the experimental group were scored positive, while only one positive instance occurred in the control group. The observed difference in frequencies is significant beyond the .001 level, thus warranting the conclusion that the experimental treatment was effective in facilitating the process under investigation.

Your interest has become sufficiently aroused at this point that you flip back to the procedure section. As far as you can tell, a satisfactory experimental design and adequate controls were employed.

Now ask yourself this question: Does it make any difference, so far as our evaluation of the evidence goes, that the results 'quoted' above are actually based on one of Reverend Loehr's experiments, and that the experimental treatment was prayer for plant growth? Most readers will answer "Yes—it makes a lot of difference!" But *should* it? This, in the opinion of the reviewer, is the most important question posed by this fascinating book. What, after all, makes prayer, ESP, and the like different from, say, verbal learning? Is it because the correlations observed in the case of the former phenomena are 'empty' in the sense that no physical process is known or even postulated? If so, what must we say about Skinner's reluctance to give his organisms any physiological stuffing? This means that the correlations he observes between S and R are

also 'empty' in very much the same sense. A good Skinnerian, no doubt, would object to this grouping of the master figure with Loehr and Rhine, but spelling out the differences in approach in some detail might prove an excellent exercise.

THE Reverend Loehr is an ordained Presbyterian minister with a bachelor's degree in chemistry. Today he is Director of Research for the Religious Research Foundation, Inc. His experiments reflect, in general, an understanding of the fundamental principles of experimentation. Care was taken to treat all plots of seedlings exactly alike in all respects except for the experimental treatment, the prayer. Plots were assigned randomly to the different treatments, of which three were customarily employed—prayer for growth, no prayer of any kind, and prayer for retardation of growth.

The results do not uniformly favor the prayed-for plants, but the trend is quite consistent, according to the author. Plots which are prayed for outgrow those which are not about two times out of three. The number of experiments in the series exceeded 700, and the number of seeds and seedlings observed was over 27,000. Unfortunately, the persuasiveness of this body of findings is undermined by the general looseness of reporting. Precise differences in growth found in individual experiments are only occasionally presented, and tests of significance are, almost without exception, not given. It would seem that scientific results in a highly controversial area such as this should be rigorously documented, and in this respect Reverend Loehr has fallen down badly. For example, only a single table is found in his book. It shows the growth for prayed-for plants to be about 106% of that for unprayed-for plants, but it is not possible to tell how typical this result may be.

The experimental results are all contained in Chapters 2 and 3 of the book's six chapters, and these two chapters will without doubt be of greatest interest to CP's readers. In Chapter 1 the author records something about his background and experiences. The last three chapters he devotes primarily to expounding the

nature of prayer as he feels it has been revealed by his research findings. Few psychologists will feel that the interpretations are closely enough tied to the empirical findings to be satisfying.

The unprofessional analysis and presentation of the data should not be allowed to obscure the basic issue posed by the book. Had the levels of confidence for the differences been computed and found to be 'significant,' we

should still have been faced with an interesting problem of what to do about 'empty' correlations or, on the other hand, what to think about levels of confidence.

#### REFERENCE

FRANCIS GALTON. Objective efficacy in prayer. In *Inquiries into human faculty* (1st ed.), 1883, 277-294. (Omitted from 2nd ed.)

## Drugs—Brain—Mind

Herbert Lippert

*Einführung in die Pharmakopsychologie.* Bern: Hans Huber, 1959 (distributed by Intercontinental Medical Book Corp., New York). Pp. 254. DM 32.—

Reviewed by HANS-LUKAS TEUBER

Dr. Teuber is Head of the Psychophysiological Laboratory of the New York University-Bellevue Medical Center. He was born in Germany, educated at the French College in Berlin, the University of Basle, and Harvard University, where he took his PhD thirteen years ago. Since then he has been at his present location, working on the psychological consequences of adult brain injuries, and the development of children, both normal and with brain defects, with a little experimental work on monkeys. The Laboratory has just got out Teuber, W. S. Battersby, and Bender, Visual Field Defects after Penetrating Missile Wounds of the Brain, and J. Semmes, S. Weinstein, L. Ghent, and Teuber, Somatosensory Changes after Penetrating Brain Wounds in Man (both published by Harvard Univ. Press for the Commonwealth Fund, 1960).

THE use of drugs to modify behavior may be as old as man himself, but psychopharmacology—the systematic study of these drug effects—is recent, so recent that it had no name until a few years ago. Lippert's *Introduction to Psychopharmacology* is probably the first attempt at monographic presentation. There is only one other book of comparable scope, H. K. Beecher's

*Measurement of Subjective Responses.* Yet Beecher is mainly concerned with drugs relieving pain, with stimulus control, and with scaling of responses. None of these things matter to Lippert. He has set himself the task of surveying the vast literature of psychopharmacology with heavy emphasis on drugs that disturb behavior—alcohol, opiates, hallucinogens—and with duly critical comments on those recent 'tranquilizing' drugs that are reputed to alleviate misbehavior. It is good to see him write so soberly on so many intoxicating subjects.

Lippert approaches his task as an intelligent physician only recently turned psychologist. His methods are descriptive, phenomenological, and literary. He covers nearly 1,700 references (only half of them German, the rest mostly English). His text gives a panorama of introspective accounts, from Baudelaire's experience with hashish to current reports of mescaline visions or of mood changes under the influence of tranquilizers. Lippert evidently believes that there is a way to find out what people feel under drugs: you ask them. They can tell us more if they are as articulate as De Quincey or Baudelaire: there will be poetry in what they tell, and always social and cultural factors—but why

not? Psychopharmacology is closer, for Lippert, to cultural anthropology than to physiology. Sir Humphry Davy's experience with nitrous oxide in 1799 clearly showed the combined effects of that gas and of the philosophy of Bishop Berkeley ("... nothing exists but thoughts!").

Such accounts make better reading than reports of 'double-blind studies' in which the subject does not know what he is getting, nor the experimenter what he is giving. But it is the double-blind study and the use of placebo controls (as Lippert himself points out) which establish psychopharmacology as a science. Without such precautions we cannot distinguish the purely pharmacologic from social and cultural effects. Lippert's sensitivity to the social factors shows in his discussions of addiction, or of the use of drugs in the procedures for interrogation and 'brain-washing.' Yet his exclusion of physiologic questions may be at the root of a difficulty which pervades the book and which he deplores in its closing section: that the studies he reviews fail to fit together, that there is no rational approach to the riddle of how chemical substances can affect the mind. We may never find out, says Lippert, unless we know what mind (Seele) really is.

It seems to the reviewer that there might have been another way: Why not substitute brain for mind? Drugs that modify behavior do so by acting on the brain. If they act differentially, they provide tools for dissecting cerebral function. To be sure, some of the presumed specificity of drug action lies in the person who takes the drug. There is the happy drunk and the sad drunk. Still, both are drunk, and drunkenness is an identifiable state. If we focus on the individual differences (in response to identical drugs), we can use pharmacology as a powerful means of testing personality tests: Can we really predict who will be a sad or a merry drunk before making them drunk? If we focus, instead, on the uniform effects of alcohol, we may arrive at a specificity that lies in the particular mode and site of interaction between drug and brain.

Lippert's expressed distaste for 'mere brain physiology' makes him neglect a

potentially unifying principle: Drugs that affect behavior produce chemical lesions in the nervous system, sometimes quite similar to restricted surgical destruction, sometimes quite different. The similarity between mescaline states and certain temporal-lobe seizures is so striking that it prompted Heinrich Klüver's brilliant discovery of the syndrome resulting from bilateral removals of the temporal lobes. The effect of the new hallucinogen LSD-25 is in some respects amazingly similar to the disturbance produced by temporary sensory deprivation. Our inquiry into the how and where of drug action touches on basic issues of neuropsychology.

The search for drug specificity (which Lippert thinks so difficult to establish) involves the same experimental techniques as the search for specific effects of restricted cerebral lesions. In ablation studies a given task may reveal deficits after frontal lesions; what is needed is some other task which will reveal effects of, say, temporal lesions. To establish specificity we must obtain dissociation of effects, so that the first task reveals deficits after frontal but not after temporal removals, and the second after temporal but not after frontal. Such double dissociation of symptoms is a minimal requirement for experimental tests of specificity, whether we study ablations or drugs. It is for this reason that psychopharmacologic experiments require more than one drug, more than one test, and more than one dosage level.

As a new experimental discipline, psychopharmacology has the vigor of a hybrid, and the potential character defects of any interdisciplinary enterprise. With no effort at all, it can become bad pharmacology—by relying on psychology alone—or bad psychology—by thinking that all of its precision lies in assessing dosages. Like any rapidly growing plant, it might grow crooked. What could pull it together more readily than a good monograph, replete with methods, critical review, lists of problems? Lippert's monograph does not quite fill this gap, even if one takes it with Beecher's book as antidote. Lippert has given us a prologue, but the book of books in psychopharmacology still needs to be written.

## Much about Management

W. Lloyd Warner and Norman H. Martin (Eds.)

*Industrial Man: Businessmen and Business Organizations.* New York: Harper & Brothers, 1959. Pp. xii + 580. \$6.50.

Reviewed by HARRY LAURENT

who is Social Science Research Advisor of the Standard Oil Company of New Jersey. His doctorate is from Western Reserve University a decade ago and, since then, he has been in personnel work in Cleveland and in planning industrial relations in Saudi Arabia. He says that the social problems of large-scale industrial management are interdisciplinary—psychology, anthropology, sociology, and economics combine.

THE list of 46 authors represented in this volume reads like a *Who's Who* of thinkers in the field of business management. Many disciplines are represented; fewer than one-fifth of the contributors are identified as psychologists. Nevertheless, there is a great deal here of interest to psychologists.

The editors, professors at Michigan State University, Warner of behavioral sciences, and Martin of business administration, wrote about one-fourth of the material. Their introduction charts the course, and their brief statements at the beginning of each chapter do an excellent job of guiding the reader from one subject to the next through this wide variety of ideas and philosophies. Along the way the reader has many views of industrial organizations, of little business as well as big business. Subjects range from personalities of successful and unsuccessful executives through the social structure of business enterprises, the goals and tasks of management, and management ideologies to some of the basic issues underlying contemporary industrial society. The original works represent a variety of publications, including a novel.

If you object to anthologies, you may



be pleasantly surprised when you read *Industrial Man*. For the most part the readings are excellent summaries with clear references to original sources for those whose interests are aroused. Only occasionally will a reader have the feeling that he missed something which should have been included.

The material is timely. Forty-two percent of the original sources were published between 1955 and 1958, another 40 percent during the preceding five-year period. Of the remainder, many are classics which deserve their space. The sources are not, however, as recent as the remaining references in a 258-item *Selected Bibliography*, more than 70 percent of which have publication dates of 1955 or later. The editors say that they consider it an honor to be associated with these contributors. I hope they will continue the association with some of them and extend it to other distinguished authors by preparing a companion volume to include selections of the many significant writings of the past few years. To me a desirable approach would be to place a greater emphasis on research and the application of research findings to the solution of problems in industry, with a corresponding decrease of the emphasis on history, philosophy, and theory.

And here are a couple of additional suggestions for the next volume. Placing the authors' biographical notes and the source references with each selection rather than at the end would save much leafing back and forth. Many of the biographical sketches include statements lauding the authors or their writings; I would prefer drawing my own conclusions on the basis of what I read.

Industrial management has changed tremendously since the days of the Browns of Providence and the Lowells of Boston and it continues to change at what often appears to be an accelerating rate. Books like *Industrial Man* serve a useful purpose by summarizing for both the business executive and the social scientist many important contributions of a period of industrial history.



*There's more to vision than meets the eye.*

—ANON.

## Social Science and the Union

Lois MacDonald, with the assistance of Murray B. Nesbitt, Peter F. Freund, and Samuel N. Seidman

*Leadership Dynamics and the Trade-Union Leader*. New York: New York University Press, 1959. Pp. xii + 156. \$2.50.

Reviewed by GEORGE STRAUSS

who is Professor of Industrial Relations in the University of Buffalo's School of Business Administration. With Leonard R. Sayles he has published *The Local Union* (Harpers, 1952), which received the SPSSI Award in Industrial Relations, and also *Personnel: Human Problems of Management* (Prentice-Hall, 1960). He is the sole author of *Unions in the Building Trades* (Univ. Buffalo, 1958).

UNTIL about ten years ago academic interest in the field of unionism was confined largely to economists. Since then a growing number of sociologists, anthropologists, political scientists, and (to a lesser extent) psychologists have begun to mine this rich area.

This book attempts to review the research to date as it relates to the crucial factor of union leadership; and it does more, for it seeks also to review all the findings of psychology and sociology as they bear on leadership generally, and then goes on to suggest how these findings may be applied to help us understand union leadership—all this within 152 pages.

As might be expected, so broad a survey is spotty. Many important studies are handled in a sentence or two. Smorgasbord on a small table gives little opportunity to savor any one delicacy. Still as a smorgasbord, as annotated bibliography, the book has much value for the advanced student, for it whets his appetite and entices him to read studies which he may have previously missed.

This area of interest is new for the principal author, who is Professor of

Economics at New York University and Director of its Institute of Labor Relations and Social Security. Of her many previous publications, most have dealt with grievance procedure and arbitration and have involved field work.

THE book is weakest in the psychological area where the author deals with "Social Psychologists, the Human Relations School, Group Dynamics, Sociometry, Small Group Research, Leaderless Groups, and the Ohio State Studies" in a few pages each and leaves the impression that none of these topics has much to offer that is relevant to union leadership. If so, why waste the reader's time? Unfortunately she makes little reference to such psychologists as Stagner, Jacques, Willerman, and the Rosens who have studied how union members perceive their leaders.

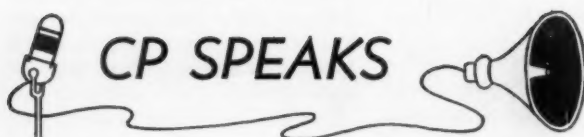
She is much stronger in handling sociological material, perhaps because she relies less on a potpourri of miscellaneous studies and more on her own insight. In any case she does a remarkably good job of integrating theory and research regarding bureaucracy, succession, and mass organization with the familiar common-sense observations of trade-union students. She points out how, as unions mature and perform ever more complex functions, they gradually harden into bureaucracies—yet not completely, for unions are "managers of discontent" and subject to the pressure of their members. Union leaders have little opportunity to exercise their leadership unless they have a cause of one sort or another to lead.

The author admits that she fails in one of the main purposes of her study: to discover in social science findings that will be of use to the union leader. Research into union life had yielded many examples which help to illustrate theory (though adding perhaps but little new theory), but so far these studies have been of little value to the union practitioner (perhaps his own fault).



*A well-used library is one of the few correctives of premature senility.*

—SIR WILLIAM OSLER



#### SARTON AND BOOK REVIEWING

**G**EORGE SARTON (1884–1956), Harvard's great historian of science, noted for his scholarship and his insistent demand for scholarly performance in others, author of the three huge incomparable volumes of the *Introduction to the History of Science from the Ancient Greeks through the Fourteenth Century* (4,245 pages, almost three million words; Williams & Wilkins, 1927–1948) and of the fact-packed but charming *A History of Science* (Harvard Univ. Press, 1952), editor of *Isis* from its founding in 1912, wrote before World War I an essay titled *Notes on the Reviewing of Learned Books*, intended for inclusion in a volume that never appeared in the form planned because the War intervened. *Isis* printed a revision of this article in 1950 (vol. 41, 149–158) and you can buy a reprint of this discussion of book reviewing by this extraordinarily competent author for 35 cents from *Isis*, University of Washington, Seattle 5, Washington. *Science* reprinted this essay last spring (Apr. 1960, 131, 1182–1187). Here, with permission from *Isis*, *CP* quotes extensively from Sarton's *Notes* and comments on them with one eye on *CP* and its problems and policies. But do not rely on *CP* for Sarton. Get the reprint and read the whole thing.

First Sarton sets the theme.

I realized very early the fundamental importance of good reviewing, because learning cannot progress without appreciation or criticism.

There you have the reason why *CP* avoids abstracting in its reviews, urges authors to evaluate the books, thus often provoking dissent and promoting thought as all dissent does.

How do you write the review?

When I have to review a book my habit is to read it in the evening, writing notes or simply page numbers on a pad as I proceed. My review takes shape during the night, and I am ready to study my notes and write the review the following morning.

How many of *CP*'s reviewers can match that? Does any of them ever read the *casus belli* in a single evening and write the review the next morning? Sarton was lucky if he could write the review before any of his thoughts had migrated whither retrieval becomes difficult. Yet it is well to do the whole job quickly when time can be spared.

It is much easier to write a review soon after having studied the book, the sooner the better.

One way to speed up is to be bright enough not to read the wrong items.

The art of reading implies the art of non-reading, and more energy is sometimes needed in order to skip rather than to continue useless drifting. Many would-be scholars never learn anything not only because they cannot read, but also because they cannot stop reading.

*CP* keeps telling its reviewers to orient the subject in the field, giving it a contemporary or historical perspective. Sarton supports *CP*.

A review should describe and characterize not only the book in question, but also the subject with which it is dealing.

That applies, of course, to the scholarly review, not to the abstract.

How long should the review be?

It is better not to write too long a review of a book, for a short review is more likely to be read than a longish one. Reviewers often ask what is the optimum size. . . . It should be possible, I think, to do justice to almost any book, that is, to give a sufficient description and appreciation of it, in a thousand words or less.

Some readers seem to think that the importance of a book is somewhat proportional to the length of the review devoted to it. That is a mistake. There is really no relationship between these two things. When a book is very good it suffices to describe it, and to praise it briefly. On the contrary, if it is defective, the defects must be explained and discussed.

There *CP* dissents. It holds that an evaluative description cannot often be deemed adequate when it is less than 500 words. For it a standard review of the run-of-the-mill is 1,000 words, although the reviewer with 1,000 words to use is apt to send in 1,300. A really important book needs, *CP* thinks, 2,000, especially if it is to be oriented in its field. A controversial book, a congeries book, or the book-of-the-year may need 3,000 or even more. This difference may arise because *CP* knows that many of its readers read the review instead of the book. Perhaps it never occurred to Sarton that a reader of the review of a very important book would be satisfied with the review and not read the book itself.

It is fair to ask that books be well written as well as that they have in them the facts that their readers will be after. Here is Sarton on that point, with his usual contempt for mediocrity.

It is not enough that a book be well built and well documented, it should be well written. There is no excuse for bad writing, which is generally a symptom of poor thinking.

As a psychologist Sarton does not fare so well. There are lots of reasons for poor writing and *tout comprendre, c'est tout pardonner*. If psychologists came in contact with scholars oftener, the motivation might arise and they would learn to write better. *CP* says that they do much better than it ever thought they would, given an opportunity and some approval. Often it is scientific intellectualism that stymies them.

*CP* keeps insisting that all critical reviewing is necessarily idiosyncratic, and that the remedy for error is dissent and rejoinder. Sarton is fully aware of reviewer fallibility. He says of himself that

he has often published favorable reviews of books that he did not like, and, what

annoyed him more, unfavorable reviews of books he personally admired. It was necessary in each case to allow the reviewer to have his own say, without hindrance. The good reviewer may write what he pleases but only upon his own signature.

The reviewers are just as fallible as the authors themselves. In spite of every precaution, they are bound to commit errors—errors of fact or judgment.

Instead of being deterred by the possibility of error, I am rather encouraged by it. If I were certain of knowing the truth, the whole truth, I would not dare to criticize anything, because my judgment would be final and inexorable. I am not afraid of expressing my candid judgment of a book, after having examined it carefully and honestly, because I know that such a judgment is at best, imperfect and precarious.

*CP*, of course, does better than this. It provides space for dissent, riposte, and rejoinder, the cybernetic approach to the exhaustion of disagreement that we call truth.

How much is the author really injured by a bad review? He alone can assess what happens to his pride, but there are both financial and intellectual ways in which an author may profit by an unfavorable review. Says Sarton:

The reviewer's judgment may help the reader in various ways. I have more than once bought a book on the strength of an unfavorable review of it.

Like *CP*, Sarton disapproves of anonymous reviewing.

According to an old tradition, reviews appear in the leading English journals without signature. Such a practice is unacceptable . . . because, in the first place, the value of the review (as of any other article) depends partly upon the qualities of its author, and secondly, unsigned reviews are credited to the editor. Now, this is nonsense, for how could the editor be held responsible for reviews of books that he has not read?

It seems that Sarton's reviewers were just as human as *CP*'s, or perhaps a little more fallible. Here is what he says about the reviewers who take the books and do not return a review and also about the intolerable procrastinators.

Much damage is done by scholars who agree to review a book and fail to do so. This is very shocking.

Even the best book needs a modicum of

publicity, for nobody will try to obtain it and read it unless he is aware of its existence. If a scholar wanted to hurt the author and prevent the reviewing of the latter's work . . . the simplest way of achieving his devilish aim would be to undertake to review it himself and then to dishonor his promise. As the review copy would be in his hands, the editor could not ask another scholar to handle the book. Such deliberateness must be rare, but the procrastination of many reviewers causes the same results, whether they be evil-minded, impotent or lazy.

So that is Sarton and it is also *CP*. Is *CP* a scholarly journal that reviews learned books? Psychology does not have enough scholars to be learned—enough scholars to write the books or to review them. Within the year *CP* has been begged by one bibliophile to give up spreading wisdom thinly over psychology's broad uneven field and to turn to scholarship, to the bibliolatriy of carefully documented technical detail, rich fare for the specialist and indigestible strange viand for the common reader. *CP* said No. In an intellectual democracy, like American psychology, there is a place for vernacular thinking, a place that *CP* has sought to fill, while breathing the more easily below the higher altitudes. Of course *CP* could be different, and perhaps sometime it should try. On the other hand, let it be said again: American psychology already has more scholarship, more erudition, and more ability to use the English medium effectively than it uses. Some of this hidden wealth emerges in *CP*, often to *CP*'s pleased surprise. How can we make scholarship more respectable in American psychology? How can we get more motivation behind it?

—E. G. B.



*Ours is a dangerous age in which the race between creative knowledge and destruction is closer than ever before. Destruction has not yet arrived, and knowledge still has a chance. Those of us who have scientific training and ability should do everything in our power to speed up creation and slow down destruction.*

—JOHN PAUL SCOTT

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# Your Personality is in Your Hands

Emilio Mira y Lopez

**M. K. P.: Myokinetic Psychodiagnosis.** (Trans. from the French by Mrs. Jacques Dubois; ed. by Leopold Bellak, Michael H. P. Finn, Leonard Small, & Frances Bishop; Foreword by Gordon W. Allport.) New York: Logos Press, 1958. Pp. xx + 186. \$6.75.

Reviewed by DAVID WECHSLER

*Dr. Wechsler is Chief Psychologist at Bellevue Hospital in New York City, as well as Clinical Professor of Psychiatry in the College of Medicine and Adjunct Professor in the Graduate School of Arts and Science, both at New York University. He is best known as the author of the intelligence scales that bear his name. He has been concerned with the psychogalvanic reflex, chronaxia, emotional reactions, and the range of human capacities. He invented the Psychogalvanograph.*

PROCEDURES for evaluating human traits in terms of body characteristics or idiosyncrasies of physiological response have always had a strong appeal to the behavioral psychologist. Professor Mira, once Spain's distinguished psychologist at Barcelona and now Director of the Institute for Professional Selection and Guidance at Rio de Janeiro, has long been interested in the development of expressive techniques for the study of the personality. His Myokinetic test (M.K.P.) is a new effort in this direction, and one of the most promising. Like other quantitative measures of expressive movement, the M.K.P. has an initial appeal of being "objective," that is to say, of requiring minimal interaction between investigator and subject. It furnishes a motor approach to the appraisal of personality and, because of its simplicity, offers the beleaguered clinician a less tortuous road to psychodiagnosis. For any or all of these reasons, one would have expected Professor Mira's ingenious test to have attracted American psychologists. Actually it has not.

Professor Gordon Allport, in his fore-

word to the present volume, expresses the view that the lack of American interest in the M.K.P. may be due in part "to the aura of charlatanry" with which graphological methods have been viewed in this country and, in part, "because studies in this area are difficult to execute." These factors, however, are at most but a small part of the story. A more immediate reason for the scant attention which the M.K.P. has received in America arises from the competition it has had to meet from already espoused projective techniques, particularly the TAT and the Rorschach. American clinicians have not only been thoroughly 'sold' on the latter, but, in this reviewer's opinion, have been unduly impressed by the role which perception plays in the structuring of personality. They have seemingly lost sight of the fact that the *persona* is determined not only by how one takes in the world but also by what one does to it. Professor Mira's researches do a great deal to reinstate the motor components of behavior as correlates of human personality.

The volume under review is Mira's third presentation in English of the theory and technique of his Myokinetic Test for Psychodiagnosis. His first paper, and in some ways the most provocative because it was accompanied by experimental data, appeared in 1940 under the title, *A New Device for Detecting Conative Trends in Personality* (Proc. Roy. Med. Soc., London, Feb. 1940). The second was in a chapter which formed part of the author's Salmon Lecture for 1942, published under the title, *Psychiatry and War*. This chapter summarized Mira's previous

study and discussed some of the more general and clinical applications of the test. The current volume is a definitive exposition of the present status of the M.K.P. as a systematic technique for appraising various aspects of personality. It is also organized to serve as a manual for the administration and interpretation of the test. Along with detailed directions for administering and interpreting the test, including some measures of reliability, the book contains a number of normative tables for different age populations. The last are of subjects examined in various South American countries.

As to the test itself, the M.K.P. consists essentially of a number of simple motor tasks in which the subject is required to make a series of movements "in the fundamental directions of space." This requirement is met by having the subject first trace lines of set length in different spatial planes (later, also some supplementary figures) and then requiring him to continue drawing the same lines with his vision occluded. He does this alternately with the right and the left hand, and in the case of several of the figures, with both hands simultaneously. The general assumption of the test procedure is that the movement of each hand "has a particular significance according to the way it is executed" and that "disturbances of psychic tension should be transferred into the domain of muscular tension provided we can eliminate the voluntary correction of the subject."

Evaluation of the subject's productions is made in terms of the differences in the length of his drawn lines in comparison with the standard. Displacements in direction of the movement are termed primary deviations; displacements to the right or left of the standard are termed secondary deviations; and degrees of angular displacement are axial deviations. The deviations are measured separately for the subject's right and left hand, the movements of which are considered to represent the dominant and nondominant side of his body. According to Mira, movements of the dominant and nondominant hands are related to the different levels of conscious and unconscious con-



trol respectively. The right is the educated, the left the uneducated hand. The former represents the acquired, the controlled; the latter, the primitive and instinctive trends. Concomitantly, deviations in the different planes (vertical, horizontal, sagittal) are associated with temperament and characterological traits. Thus, positive primary deviations in the vertical plane signify elation, negative deviation signifies depression. A positive deviation in the horizontal plane shows extratension, a negative deviation reveals intratension. Positive deviation in the sagittal plane is indicative of heteroaggression, negative deviation of autoaggression. Interpretation of these indicated traits depends, in addition, upon whether the deviations are manifested more intensively by the left or right hand. In general, traits inferred from the movements of the left hand are interpreted as constitutional and deep-rooted; those of the right hand as temporary and culturally determined.

The foregoing trait assessments are derived primarily from the execution of the initial portion of the test, namely, from the drawings of the lines in the different planes, and are referred to as the *lineograms*. The other parts of the test involving reproduction of various figures such as the *zigzag*, *chains*, *staircase*, etc., are used primarily to confirm the information already obtained from the lineograms, although in some instances they also furnish additional diagnostic material. In the reviewer's experience, execution of the added figures consumes much more time than the basic lineograms, and his feeling is that most of the supplementary figures could be omitted without seriously impairing the value of the test. The time factor is of moment not only in the matter of administration but also in the scoring of the test. Mira mentions some 79 recommended measurements, many of which are difficult to make and for the most part are unsupported by available norms. Some of the recommended measures may be of value for research purposes, but in the practical application of the test the reviewer has not found most of them utilizable. The test would gain much as a clinical instrument if substantially shortened.

One regrettable weakness of the book is the author's somewhat cavalier presentation of his theoretical formulations, which are often cryptic and not infrequently hard to follow. Some of the difficulty in comprehending parts of the text may be due to strict linguistic fidelity to which the editors seemingly committed themselves in translating from the original Spanish or French versions. They note that "translation difficulties arose in which the balance might depend upon a single word that meant nothing to us." As one who has had similar experience in reading Mira



EMILIO MIRA Y LOPEZ

on the M.K.P., I can sympathize with them, but I think they would have done much better by the author, as well as the reader, if they had felt free to paraphrase rather than strictly to translate Mira's expositions. One also has the impression that the author and the editors could have made out a better case for the predictive potential of the test by presenting in some detail the findings of other investigators.

The book has a bibliography of some 127 titles, but very few of them are effectively utilized or referred to in the text. It is to be hoped that the promised forthcoming volume by H. Michael Finn will contain a fuller and more systematic review of the literature.

Altogether, the M.K.P. constitutes a basic contribution to the evergrowing field of projective tests. It is rooted more firmly on the objective performance of the examinee and less subject

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to arbitrary interpretations by the examiner than are most other projective techniques. Moreover it deals with and brings to light aspects of the personality which sensory-perceptual approaches often fail to reveal. M.K.P. has the important virtue of being independent of content and relatively unfettered by interjacent symbolism. While Mira states that the test is influenced by cultural factors, that can be true only in a small degree. Many of his claims for M.K.P. need to be confirmed and, while some of the findings reported, like the high correlations (.75) of the M.K.P. and intelligence, seem spurious, the over-all validity of the test as a clinical tool is amply supported.

## The Birth of Death Psychology

Leroy Bowman

*The American Funeral: A Study in Guilt, Extravagance, and Sublimity.* (Introduction by Harry A. Overstreet.) Washington, D. C.: Public Affairs Press, 1959. Pp. x + 181. \$4.50.

Reviewed by DONALD G. PATERSON

who is Professor of Psychology, just become Emeritus, at the University of Minnesota, where his special concern for many years has been applied psychology, occupational and vocational, questionnaire and interviewing techniques, psychometrics and individual differences. For twelve years he was the editor of the *Journal of Applied Psychology*. Right now he is advising CP about popular books, whether they should be reviewed or not, whether they help or harm the public.

THE author of this book has a PhD, presumably in sociology, and a long experience in settlement-house work and in teaching. The publisher's blurb says that the book describes Bowman's attempt to apply scientific methods, to-

gether with a philosophical and practical approach, to a neglected area of study. An introduction by Harry A. Overstreet points out that funerals have not been among the happiest of our human inventions—rather something about which many are becoming increasingly embarrassed.

Anthropologists have long delved into the mysteries of tribal customs connected with death. Sociologists have seldom made the attempt to study such an institution in modern, urban, industrialized society, and this book is a noteworthy exception. Psychologists have done even less.

The study is a product of Bowman's life-long interest in the subject coupled with observations and interviewing in two small villages, in two towns of 5,000 population, in one city of 185,000, one of 340,000 and in three metropolitan areas of over one million each. In addition, Dr. Bowman conducted over a hundred group discussions. Judging from the way his data are presented and discussed, the method approximates that of the roving reporter. That the result could be labeled a scientific treatise is not to be expected. Nevertheless it is worth while as an account of contemporary attitudes and practices in all their variety and ramifications.

As a topic for open discussion, death and funerals remain today about as much tabooed as was sex a generation ago. In the reviewer's opinion, it is a sign of healthy progress when topics that were formerly swept under the rug are brought out into the open for public airing, even though it is doubtful that this book will do for death and funerals what Kinsey did for sex.

Bowman begins by describing the anomalies, the ambivalence, and escapist attitudes toward the topic itself. Then follows a description of the reactions of his family and friends when death strikes down a loved one. Intense feelings of guilt often arise, even though groundless. From these may stem a prevailing emphasis on 'dolling up' the corpse, public viewing, and extravagant expense. The third chapter describes group behavior at funeral gatherings.

The next section of the book contains five chapters on the funeral business as

a commercial enterprise and the attempts of the undertakers to achieve status and to secure laws that will protect their vested interests. The third section discusses changes in the forms and functions of funerals with deserved emphasis on contemporary trends and experiments. A final section makes a plea for elimination of the evils that beset this gruesome business through adoption of a more scientific and rational approach to all the problems that arise on the occasion of death. In this plea Bowman is at his best as he attempts to persuade us to shift the emphasis from tawdry display to a dignified means of stressing the psychological, social, and spiritual meaning of life's termination.

DURING his lifetime, the reviewer has found it fascinating to observe the expanding horizon of research interests and activities of American psychologists. Beginning as specialists in introspection, psychologists used their fellow graduate students as subjects in experiments. A bit later they built what McNemar has called the psychology of the college sophomore. Now they have moved to the intensive study of behavior at each age from the foetus to the geront. At last it is time for them to extend their studies to the terminal stage of life itself and to the reactions death precipitates in the surviving. Perhaps shortly we shall be hearing about the mortuary psychologist as we now hear about the activities of the child psychologist.

Here are some of the topics demanding study by the best research techniques: attitudes and opinions of various segments of the population toward undertakers, undertaking, and kinds of funeral arrangements, including cremation and dispensing with funerals altogether; the recalled experiences and present attitudes of the bereaved six months to a year after bereavement; opinions and attitudes of physicians, clergy, attorneys, legislators, and journalists toward the behavior and practices of the undertakers; the needs of the bereaved for psychotherapy; the training of bereavement counselors; and perhaps above all problems associated with education in the calm and unpro-

testing acceptance of the fact of mortality and in planning for and meeting death—certainly a neglected area of education.

In the Extension Division of the reviewer's own university, as in five other colleges and universities, there is a fifty-two year old Department of Mortuary Science. (There are also 17 proprietary schools in the United States.) At first the length of training was six weeks. In 1916 the curriculum was extended to eight weeks and later, successively to 12 weeks, 24 weeks, and 36 weeks. In 1951 the curriculum was expanded to a three-year program, which requires full-time attendance covering nine quarters of work totaling 134 quarter-credits with only a modicum of 'liberal' education. Admission is restricted to graduates of an accredited high school or its equivalent. Graduates receive the degrees of Associate in Mortuary Science. A course in beginning psychology is offered in the first year and a course entitled *Psychology of the Funeral Service* is offered in the second year. The description of this latter course is: "Principles helpful to a prospective funeral director in dealing with his clients, especially those under severe emotional stress." The instructor of this course also teaches *Introduction to Embalming*. Perhaps, when the curriculum is expanded to four years and post-graduate work is also required with a possible graduate degree, psychological research will also be welcomed.

In the meantime there is urgent need for bona fide psychologists to turn their serious attention to this serious but sadly neglected subject. A research team with representation from social psychology, clinical psychology, vocational psychology, economics and political science would be needed to bring all aspects of its subject out into the open for rigorous study. Bowman's book may prove helpful as a stimulus to rational interest in a subject which sooner or later, perhaps often, touches the life of every one of us.



*A fanatic is a man who can't change his mind and won't change the subject.*

—WINSTON CHURCHILL

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# Systematic Psychoanalysis as Research

Dexter M. Bullard (Ed.)

*Psychoanalysis and Psychotherapy: Selected Papers of Frieda Fromm-Reichmann.* Chicago: University of Chicago Press, 1959. Pp. xiv + 350. \$7.50.

Reviewed by ALLEN T. DITTMANN

*Dr. Dittmann is a clinical research psychologist in the Laboratory of Psychology of the National Institute of Mental Health. Like the author of this book he is a neo-Sullivanian (Harry Stack), but he is more analytic in his research than she. Just now his research is centered on finding out how two people communicate with each other—how, and how much, and when, and maybe why.*

NEXT to its founder, Harry Stack Sullivan, the Washington School of Psychiatry is perhaps best known through the writings and teachings of Frieda Fromm-Reichmann. Indeed she counted Sullivan as among the teachers who had influenced her the most, along with Freud, Goldstein, and Groddeck. Fromm-Reichmann came to the Washington area from Heidelberg via Alsace in 1935, ostensibly to spend two months at Chestnut Lodge, a private sanitarium outside of Washington, of which Dexter Bullard is Medical Director. Her stay turned out to last 22 years until her death in 1957. The exploration of ideas was always exciting to her, and she counted her year at the Center for Advanced Study in the Behavioral Sciences as among her most rewarding experiences. *Psychoanalysis and Psychotherapy* was compiled by her colleagues, friends, and students as an expression of their wish "to preserve and concentrate Frieda Fromm-Reichmann's thoughts and ideas." It contains virtually all of her publications in English. Twenty-seven earlier papers in German are listed by title for reference.

The 23 papers in this book are arranged under six subheadings. They

range from general discussions of the philosophy of psychiatric disorders and their treatment to a tentative attempt to describe a single feeling-state. The decision to include every article is understandable in a memorial volume, yet it leads to a book which is in some areas repetitious. It also leads to unevenness in level of conceptualization: some papers are addresses before general medical audiences, whereas others are serious, scholarly reviews of literature or of clinical experience. The chronological arrangement of the articles within the six sections, however, gives the reader a chance to see the development of Frieda Fromm-Reichmann's thinking over the years, and it is this development to which I should like to address this review.

THE evolution of Fromm-Reichmann's ideas is most visible in her papers on the treatment of schizophrenic patients. This is not surprising in the light of the amount of time she spent with these patients over the years. She was a psychotherapist of unflinching devotion, and the real surprise is that she had any time left over for teaching or reflecting on her work. For most psychotherapists clinical activities are designed to be useful to patients, and reflecting is designed to increase that usefulness. Certainly Fromm-Reichmann worked under these motivations as much as any clinician, but she had an additional goal as well: clinical research. The sort of research she conducted has been variously looked upon by psychologists: some would not call it research, because the investigator does no counting of cases or calculating

of chi squares, whereas others are awestruck by its complexity and by the importance of the problems it tackles.

Let me give some content to this discussion. In the first paper in the section on schizophrenia (published in 1939), Fromm-Reichmann hypothesizes that the patient has withdrawn because early experiences of rejection and neglect have led him to be wary of people in the present because the risk of repeating old hurts is too great. The psychotherapist, therefore, must treat the patient with the utmost care, trying to supply the warmth and nurturance which was once missing. By 1948, however, Fromm-Reichmann had learned that her initial formulation was incomplete. The early type of treatment seemed to suggest to the patient that the psychotherapist considered him childish, or worse, that the therapist was overly cautious because he feared the patient's 'craziness'—and this from the very person who was supposedly offering help. Experience seemed to show that while there were childish or regressed aspects to the schizophrenic patient's make-up, even the most 'deteriorated' patients had grown up in some areas. Therapeutic interventions addressed to these more adult parts seemed to be more effective.

This change in point of view was developed further by 1954, when Fromm-Reichmann postulated that the schizophrenic patient's withdrawal and initial aloofness were not only the fear of rejection but also the result of intense anxiety over secret hostility toward persons on whom he might feel dependent. Thus the patient seeks to protect himself in advance from the consequences of his rage by refusing to enter into the relationship in the first place. According to this formulation the therapist must focus upon the relationship between himself and the patient. The work of psychotherapy becomes an exploration with the patient of the dynamics of his symptomatology in terms of defense against anxiety as exemplified in that relationship.

This outline is necessarily schematic. What is there in it that makes it research? The essence of the research entails three processes: (1) Fromm-Reichmann describes in clear, communicable terms her formulations of the schizo-





FRIEDA FROMM-REICHMANN

phrenic disorder and of the treatment which seems to follow from this formulation. (2) The fact that she has formulated, however, does not close her eyes to new facts which seem to run contrary to what she has said before. (3) She alters her formulations in the light of new facts, and describes the revisions in equally clear terms, which are in turn subject to further revision with further experience. The changes are not inconsistencies in thinking, but seriously considered alterations which are forced upon the thinker because the facts, in this case patients, refuse to behave the way the initial formulations said they should.

An older and by now famous example of this sort of clinical research may be found in the drastic reformulation to which Freud subjected one of his early ideas. In 1896 he wrote that a passive sexual experience before puberty is the specific etiology of hysteria. By 1905, however, he had found that many of these reported experiences were fantasies. He had also found that there were many similar experiences in the histories of normal people. The etiology of hysteria now seemed more complicated and had to be reformulated.

**T**HE methods of clinical research discussed here are fundamentally anecdotal, and we might well ask if the addition of other, more systematic, research techniques could sharpen or speed up the whole process. This is a hard question to answer. Carl Rogers made con-

siderable alteration of his formulation of the working of psychotherapy between *Counseling and Psychotherapy* in 1942 and *The Necessary and Sufficient Conditions of Therapeutic Personality Change* in 1957, and he had the more systematic methods at his disposal. Indeed he cites a good deal of research as he goes along. But in his 1957 article he mentions research chiefly to indicate that concepts like those he talks about are subject to measurement and thus to future research. The influence of research on the development of Rogers' basic thinking about how to do psychotherapy is, however, less specific and easy to point to. It seems as if the formulations must come first, via the more anecdotal, clinical research, while more systematic research must follow, taking as its task the testing of the clinical researcher's conclusions. But this is a paradox: it makes it look as though systematic researchers in the field of psychotherapy could not be creators of ideas, whereas in many other fields quite the reverse has been true. Certainly it points up the difficulties of making hard, measurable variables out of ideas which cover such a wide range of human interaction as those involved in psychotherapy.

Thus Fromm-Reichmann's articles, especially those on schizophrenia, give a case history of clinical research at its best. I cannot close this review without referring to some of the more personal feeling which shows through much of the book. It is best exemplified by her final article, one on which she was working at the time of her death. Its title, *On Loneliness*, might well have been *Beyond Anxiety*. It shows an inquiring mind approaching a feeling-state which is by its very nature unapproachable. Loneliness seems to be that state of complete isolation which everyone fears more than anxiety: the feeling of being cut off, seemingly forever, from those contacts which make humans human. Schizophrenic patients try to describe it after they have been there, but can only resort to cryptic phrases or poetry. The fear of loneliness, the fear of being enveloped by that nameless state, may be what really makes people afraid of schizophrenic patients, makes them think of these patients as 'out of this world'

or as a different species than the rest of us. Frieda Fromm-Reichmann writes more warmly in this paper than she does in any of the others. From it the reader can feel how really deeply she tried to understand her patients in all their experiences, even the most terrifying ones.

## What Happens to Cerebral Palsy Later

Bronson Crothers and Richmond S. Paine

*The Natural History of Cerebral Palsy*. Cambridge, Mass.: Harvard University Press, 1959. Pp. xii + 299. \$6.75.

Reviewed by HELMER R. MYKLEBUST

who is Professor of Language Pathology and Director of the Institute of Language Disorders in Northwestern University's School of Speech, as well as Professor of Psychology and Professor of Neurology and Psychiatry, each in the proper Faculty. His interest in brain-damaged children dates back to Edgar Doll at Vineland, New Jersey. He got his general training from Rudolph Pintner and he also had contact with Herbert Langfeld and Carroll Pratt at Princeton. It was Pintner who interested him in deafness, and he has published *Auditory Disorders in Children* (Grune and Stratton, 1954) and very soon will have published *The Psychology of Deafness: Sensory Deprivation, Learning, and Adjustment* (also Grune and Stratton, 1960). Where Dr. Myklebust works, they think "CP" means Cerebral Palsy, not even Communist Party.

**T**HE late Bronson Crothers, the senior author, was Clinical Professor of Pediatrics, Harvard Medical School and Senior Consultant in Neurology at Boston's Children's Medical Center. Richmond Paine is Associate in Pediatrics, Harvard Medical School and

Associate Physician in Neurology at the Boston Children's Medical Center. This book reflects extensive experience and it is fortunate that these men joined efforts to provide this scholarly, useful volume on children having disorders of the central nervous system.

There have been many unilateral studies of the handicapped covering a contact with each subject but investigations covering more than one contact, including follow-up appraisal of initial findings, classifications, and conclusions, are rare. Crothers and Paine saw their patients as young children and again after many of them were adults. Their study can be described as a major contribution to the field of cerebral palsy with the findings and conclusions having broadly generalized implications.

There is something of importance in this volume for the clinical psychologist, neuropsychologist, speech pathologist, audiologist, and the psychologist concerned with special education and learning. The point of view is realistic, objective—yet dynamic, cautious, and critical; it is encouraging, if not optimistic. The book's emphasis on the psychologist's role and his contribution in a neurological setting, with its frank recognition of the limitations of interdisciplinary work, is refreshing as well as beneficial.

The study entailed contact with over 1800 patients, of which 561 constituted the basic research population. The patients were re-examined fully, having been first seen in childhood, in an attempt to ascertain the effects of cerebral palsy on growth and development. Most of the subjects were young adults. The major findings are reported under the categories of motor involvements, etiologies, seizures, EEG findings, intelligence, life expectancy, employability, treatment, education, and emotional status. While all aspects of this study are of interest, this review will limit itself to those most directly related to the psychologist's world.

**T**HE most common type of cerebral palsy is spastic hemiplegia in which in the child involvement of the arm is noted before that of the leg; most children classified as monoplegic in infancy turn out to be hemiplegic when ex-

amined later. The next highest frequency is those with extrapyramidal involvement. When patients are classified on the basis of the type of cerebral palsy and studied appropriately, important differences appear. The groups might vary according to the common etiology, intelligence, emotional status, prognosis, seizure onset and frequency, and employability. Some specific findings are these: mental deficiency is more common in the hemiplegic; the left hemiplegic patients are more intelligent than the right; there is a correlation between convulsions and impaired intelligence; speech defects are equally common in right and left hemiplegics; the hemiplegic child is not delayed in walking and his onset of seizures often falls between one and four years.

The child with extrapyramidal involvement most often is seen first because of his inability to learn to sit; the most common motor disorder is athetosis; dysarthric speech, drooling, and facial grimacing are characteristic. Of the cerebral palsied this group is the most intelligent and least often requires institutionalization. An outcome which impressed these investigators was that most of the successful adults had rebelled against their advisers from time to time, had experimented on their own, and had made independent decisions.

More generally, these authors conclude that in the past attention has been devoted to the motor disorder without due regard for total development. Because treatment is effective in early life is no reason to assume that the same treatment is advisable as the individual matures. Unless the focus of attention is widened beyond the physical problem, docility and overdependence may be fostered. Insufficient attention has been given to reappraisal and to the abandonment of treatment when it becomes ineffective as the child grows into adulthood.

Treatment for the motor disorder is reasonably satisfactory for those having spasticity but there is little evidence that these procedures are beneficial for those having extrapyramidal involvement. For all the cerebral palsied there is reason to believe that the motor and

the intellectual problems are only a part, and at times but a small part, of the total circumstances confronting the individual. Often more important are parental attitudes and the emotional conflicts of the person. Many were bitter, resentful, and unduly dependent. A common problem was the perfectionistic demands of the parents.

While the content of this volume is not easy to digest, its challenge and general interest make its absorption a rewarding experience. It is recommended wholeheartedly.

## The Mass Man and the Other Kinds

William Kornhauser

*The Politics of Mass Society.* Glencoe, Ill.: Free Press, 1959. Pp. 256. \$5.00.

Reviewed by JIRI KOLAJA

*who is Assistant Professor of Sociology at the University of Kentucky. He was born in Czechoslovakia, became an American citizen later, has a PhD from Masaryk University in Brno and from Cornell University in Ithaca, and will publish shortly a book called A Polish Factory, based on a field study in Poland three years ago.*

**S**OCIOLOGY of the last two or three decades has been characterized by a growing reluctance to develop large society theories. In a good Cartesian tradition, the tendency has been to concentrate upon smaller and therefore more manageable units—the small group, the community, the organization. Recently, however, Talcott Parsons has moved from the theory of action seen predominantly as action of one actor to the theory of economic systems and societal levels. The book under review moves in the same direction. Whether this is a signal of a certain change in sociological interest or merely an accident remains to be seen. This reviewer believes that this move is desirable, for the proper subject of sociology ought to

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be, in the first place, the study of the total society.

*Mass society*, as opposed to the totalitarian, communal, and pluralist society, is defined by Kornhauser as a "social system in which elites are readily accessible to influence by non-elites and non-elites are readily available for mobilization by elites" (p. 39). *Accessibility* is roughly defined as the degree to which non-elites participate in the selection of elites, while the *availability* of non-elites is indicated by the extent to which members of the society lack attachments to independent groups. The theory maintains that, within the modern mass society, there has been an increasingly greater absence of intermediary groups which can mediate between primary groups (like the family) and the tertiary level (the nation and the state). Such intermediary groups (the secondary level) would be represented by the local community, voluntary organizations, and occupational groups. Thus it is not the disorganization of the family, as such, but rather its isolation from the larger society that accounts for the development of the mass society. In a pluralistic society, on the other hand, there is a greater number of intermediary groups; consequently stimuli provided by the elites are mediated and checked by these intermediary groups. France, with its more centralized national government, would provide an example of a society that is closer to the type of a mass society, while English-speaking countries with their more developed local systems of government and many voluntary organizations approach the constructed type of the pluralistic society. Furthermore, the effect of a sudden change from a communal agricultural society (low accessibility of elites and low availability of non-elites) to a modern industrial society is more likely to develop a mass society than if a gradual change is present. That is because the relatively discontinuous change does not provide time enough for the development of intermediate groups, so that a social alienation of the individual from his society results.

For the four types of society Kornhauser identifies four psychological types: mass man, traditional man, totalitarian

man, and autonomous man. The mass man is defined as a person who lacks a strong set of internalized standards and an acceptable self-image. Therefore "he seeks to overcome the anxiety accompanying self-alienation by apathy or activism" (p. 112).

To support his theory by data, Kornhauser falls back upon several opinion survey and political preference studies undertaken in America or in Europe mostly during the years 1948-1958. Though the lower strata are more responsive to mass appeals than are the higher, within all strata those groups with the fewest social ties are asserted to be the most receptive to mass appeals characterized by undemocratic values.

Now, turning to a critical evaluation of the theory, let us first point out its genuine sociological nature. Similar to Durkheim who sought to explain, for example, suicide in terms of differential intensity of the social tie between the individual and society, the mass society is explained in terms of frequency of social ties and number of intermediary groups within the system of societal levels. Thus Kornhauser should be commended not only for his concern with the total social system, but also for a development of a genuinely sociological scheme. Characteristically, he enters upon a weaker foundation in his theory of "psychological types." Provided that the mass man is rooted in his primary group family and friends, as is maintained by the author at other places (e.g., p. 92), there is no strong reason for the mass man to develop the self-image described above. Likewise the description of the totalitarian psychological type seems to be inadequate.

In respect of the thesis about the increasing absence of intermediary groups as represented by France (and to some

degree adumbrated to be appearing also more in the pluralistic American society, p. 234), we may note that nowhere in the book has it been shown that the number of the intermediary groups has actually decreased. Rather it is maintained that their importance has decreased. The reviewer suspects that, as far as occupational organizations are concerned, even in France their number has considerably increased as compared, say, to some fifty years ago. In America, though the statistics lose their reliability as we move to earlier records, it appears, nevertheless, that, in addition to occupational organizations, the number of local churches and their membership have more than proportionately increased.

In the conclusion of the book the author has shown a strong preference for the pluralistic society whose system of multiple groups and multiple membership in different groups is said to secure a certain anti-totalitarian equilibrium. Though the reviewer shares the same preference of values, he cannot omit pointing out that in a Communist society, members of the Party who subscribe to non-democratic values usually belong to several organizations, while democrats withdraw from participation in organizations as much as they can afford it. Thus we not only qualify the theory of the pluralistic society but must also be on guard lest we slip in tacitly certain culture-bound value judgments.

The book definitely represents an achievement which is above the average. The author has not only developed a promising theory but has also displayed considerable skill in relating data collected in different countries for purposes other than the corroboration of his theory. A follow-up study explicitly designed to test the theory of the mass society would be, of course, desirable.



*The world knows next to nothing about the natural mental capacities of the female sex. Only after generations of civil freedom and social equality will it be possible to obtain the data necessary for an adequate discussion of woman's natural tendencies, tastes, and capabilities. . . . The [Harvard] Corporation do not find it necessary to entertain a confident opinion upon the fitness or unfitness of women for professional pursuits.*

—CHARLES W. ELIOT (1869)



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# When Symptoms Yield to Drugs

Henry K. Beecher

*Measurement of Subjective Responses: Quantitative Effects of Drugs.*

New York: Oxford University Press, 1959. Pp. xvi + 494. \$12.75.

Reviewed by MURRAY E. JARVIK

*The author, Dr. Beecher, is Dorr Professor of Research in Anaesthesia in Harvard University and Anaesthetist-in-Chief at the Massachusetts General Hospital. He has long been interested in the pharmacology of anesthesia, in the action of hypnotics and narcotics, and in surgical shock and pain. The reviewer, Dr. Jarvik, is Associate Professor of Pharmacology of the Albert Einstein College of Medicine in New York City. He has an MD and a PhD, and bears the marks of intellectual stimulation by Egon Brunswik and Edward Tolman at the University of California in Berkeley and later of Karl Lashley at the Yerkes Laboratories. He is now noting the psychological effects of drugs on monkeys.*

“DISEASE manifests itself by abnormal sensations and events (symptoms), and by changes in structure and function (signs). Symptoms, being subjective, must be described by the patient. Signs are objective.” Thus begins a widely used textbook of physical diagnosis (Cabot and Adams). In contrast to the common pessimistic view that symptoms are difficult to measure because they are subjective and qualitative, Beecher attempts to demonstrate how they may be quantified with pharmacological agents. Although his stated purpose in this volume is “to show that a quantitative approach to sensation (feeling) is possible and rewarding,” his orientation is quite different from that found in classical psychophysics in that the focus of interest lies in the effect of drugs upon symptoms rather than in the relation between stimulation and sensation.

It is clear that the ability to measure quantitatively the relief of symptoms

should be valuable to medicine. In showing how to accomplish this end, the book makes a worthwhile contribution to psychopharmacology, particularly in the exposition of the design, conduct, and interpretation of experiments in this field. The statistical techniques discussed in Chapter 4 (prepared by Frederick Mosteller) include examples of possible mishandling of data and should be especially useful. The conscientious use of controls in experiments, as employed in Beecher's own anesthesia laboratory at Harvard, and the excellent discussion of placebos and double-blind procedures can serve as a model for the design of experiments dealing with the effects of drugs on human behavior. Current medical literature abounds with uncontrolled clinical work with drugs and this work might have a salutary impact on the investigators.

Complaints of physical discomfort are among the commonest in medicine, so it is appropriate that the first half of the book should be devoted to the measurement of pain. Beecher feels that “significant” pain, unlike vision or hearing, cannot be made readily to yield a psychophysics because the adequate stimulus is not easily identifiable. Nevertheless he prefers to use naturally occurring pain (e.g., operative trauma, cancer) as an unmeasured stimulus for the verbal reports which might be influenced by drugs (e.g., morphine). These situations he feels have a pathological meaningfulness not reproducible in the laboratory and have already led to successful studies of analgesic drugs. Nevertheless his criticism of the use of contrived or experimental pain as a means for testing drugs seems unduly harsh. For example, he says that it is difficult to reproduce reliably the origi-

nal drug results of Hardy, Wolff, and Goodell, yet he makes no mention of the work of Birren and of Bindra that verifies the reliability of this method. Beecher's attitude is reminiscent of Egon Brunswik's view that “representative” design is more likely to reveal relationships involved in natural functioning of an organism than are contrived experiments, that eventually it may prove to be the most profitable approach.

From a practical standpoint it is remarkable how precisely this author, utilizing only verbal reports from patients, has been able to assay the analgesic potency of various drugs. Although his technique is analogous to the psychophysical methods, his physical unit is not a stimulus intensity but a milligram of morphine. This method has, in fact, been adopted by the Committee on Drug Addiction and Narcotics of the National Research Council.

THE knottiest problem in the book is posed by the interpretation of subjective reports of pain by patients. There can be no doubt that such reports may accord quite well with a number of other variables. Is it necessary to assume that a patient has a clear concept of what is meant by *pain*, a word he probably learned in childhood, simply because he can reliably identify some effect of a drug, an effect which may or may not be analgesic? It seems that Beecher implicitly defines pain as that state, determined by verbal reports, which is diminished or abolished by morphine. It is uncertain whether he would accept this interpretation of his concept of pain as accurate or restrictive since he also uses the term in a number of other contexts. He distinguishes between “original sensation” and “reaction” and indicates that the latter component is much more susceptible to pharmacological, psychological, and neurological (lobotomy) influences. His distinction between psychic and motor reactions (p. 176) may disturb psychologists pledged to objective descriptions, and comparative psychologists may disagree with his assumption that animals cannot communicate a subjective experience (p. 93). He concludes (p. 188), however, that “pain cannot be satisfac-

torily defined except as every man defines it introspectively for himself."

In the second half of the book, Beecher describes the influence of drugs on a variety of sensations and perceptions. It is surprising that in the chapter dealing with anxiety he does not mention the new tranquilizing drugs, many of which are now widely prescribed to relieve anxiety. His method would certainly lend itself most effectively to an evaluation of such relief. In another chapter (p. 257), he does, indeed, comment disapprovingly on the use of the term *tranquilizers*, but his view that the classification is not logical or homogeneous might apply equally well to sedatives, hypnotics, and general anesthetics. Since the book appears to be a selection of illustrative examples rather than an exhaustive review of the subjective effects of drugs, this omission cannot be considered serious.

Beecher's stimulating iconoclastic approach to a number of apparently old and well-settled problems is sprinkled throughout the book. For decades morphine has been accepted as a necessary routine pre-anesthetic medication. Yet Beecher's well-controlled studies show that both anesthetists' and patients' ratings do not show any superiority of morphine over pentobarbital given before surgery. In addition, he gives evidence that a dose of morphine produces more deleterious physiological changes than an equally effective dose of pentobarbital, as judged by the subjective relief of apprehension. Elsewhere he indicates heretically that meperidine, which has replaced morphine in conditions where respiratory depression would be undesirable, in fact produces as much respiratory depression as morphine when given in equianalgesic doses. These are only a few of many instances in which the author attempts to demolish traditional ideas. Well-established views die with difficulty and one can only hope that Beecher's healthy scepticism may stimulate others to corroborate his findings.

ONE shortcoming of this book is the uneven treatment of the problem of face validity. This reviewer feels that the systematic use of external criterion groups, such as those employed for the

validation of the Minnesota Multiphasic Personality Inventory, would enhance the value of Beecher's tests. In one chapter Beecher points out the significant finding that codeine, given to patients as an unknown, causes them to report a reduction in frequency of chronic cough, whereas, in fact, an objective count shows no change. On the other hand, in a different chapter, he concludes that morphine causes mental clouding because answers to questionnaires indicate such an effect. Elsewhere an experiment of von Felsinger, Lasagna, and Beecher is cited in which four out of ten patients under LSD are said to have experienced an increase in anxiety and two had erotic sensations. The critical reader might be curious to know how these states were measured, but unfortunately such information does not appear in the book.

There is plenty to argue about, a good deal to think of, and much to be impressed with in this book. It is a useful reference work on the measurement of pain, though it is a far from impartial review. Enough facts are presented to allow the reader to decide whether the lauded methods are as good or the disparaged ones as bad as Beecher thinks they are. The author and his colleagues and students have cleverly handled the problem of measuring the ability of drugs to produce subjective effects. These include "mental clouding" (author's quotes), sedation, euphoria, introspective aspects of anxiety, hunger, nausea, pruritus, and cough. Each symptom complex is described in the series of collected essays that constitutes the second half of the volume. The book is recommended reading for anyone dealing with the effects of drugs on these symptoms, and this advice includes physicians and pharmacologists. Psychologists might be pleased to read how some of their methods are infiltrating medical science. The net effect of reading the volume is admiration for the tenacity and success with which Beecher has attacked so many slippery psychopharmacological variables at the same time.



## Social Psychology for Clinical Training

Abraham S. Luchins

*A Functional Approach to Training in Clinical Psychology: Via Study of a Mental Hospital.* Springfield, Ill.: Charles C Thomas, 1959. Pp. xlvii + 288. \$7.50.

Reviewed by JOSEPH D. MATARAZZO

who is Professor of Medical Psychology and Head of the Division of Medical Psychology in the University of Oregon's Medical School and who has already written several reviews for CP. See, for example, his review of McGhie's *Psychology as Applied to Nursing* (Williams & Wilkins, 1959; CP, Mar. 1960, 5, 90f.). He himself is a teacher-supervisor of students in clinical psychology at Oregon and he says that his work is going to show the effects of Luchins' persuasive propaganda.

THE author of this book, Abraham S. Luchins, is Professor of Psychology at the University of Miami. For the past 15 years, he has authored numerous articles championing more rigorous training in experimental psychology for students of clinical psychology. A decade ago, when friction between clinical and experimental psychology was rife, his articles espoused an experimental clinical psychology. In all this writing, he drew heavily upon his intimate knowledge of experimental, clinical and social psychology. *A Functional Approach to Training in Clinical Psychology* reflects even more clearly than did these articles the author's firsthand acquaintance with both the literature and practices of the three areas of psychology.

In 1955, while serving as a consultant in psychology to a state hospital, Professor Luchins had an opportunity to organize into one unified training program the various single training projects with which he had experimented previously at various universities, army

installations, state and Veterans' hospitals. The present book outlines this unified and comprehensive intradisciplinary and interdisciplinary field training program for students of clinical psychology.

The main feature of the training program is its organized plan for a thorough and penetrating sociopsychological study of a mental hospital (or other clinical installation). By use of this analysis, the goal of the training program is to produce clinical psychologists who are, first and foremost, well-grounded students of human behavior. Clinical skills and activities are viewed as a special application of the principles of general experimental and social psychology.

Unlike contemporary programs for internship in clinical psychology, Professor Luchins' schema does not envisage a clinical installation as a place where students practice diagnostic and therapeutic skills. Instead, it views a clinical facility as a place where students learn about the installation itself: about its physical and geographical features; its functional units; its relation to the community, to government, to public policy and public opinion; its administrative and social organization; its networks of communication; the positions in it, the people who fill the positions, the roles they play, the problems they face; and the socio-cultural-institutional context of the clinical phenomena manifested by patients with whom psychologists and other hospital personnel work.

**S**PECIFIC objectives of the training program include the following. It seeks to expose the student to the total hospital setting and to allow him to become intimately acquainted with its structure, activities, and problems. It seeks to help the student to comprehend the administrative organization and the day-by-day operation of the hospital, to become acquainted in a meaningful sense with all levels of hospital staff, their duties, responsibilities, personal and professional attitudes, and their relationship to one another and to patients, to learn to work and cooperate with hospital staff (other than psychologists and psychiatrists), and to write psychological reports for differ-

ent consumers which are meaningful to them. Unlike the traditional training programs, the present one encourages the student to get a picture of the hospitalized patient and his habitat not only during the hour or so that he may spend in formal diagnostic or psychotherapeutic activities, but also during the other 23 hours of his life-space, to see the psychologist's activities in their proper place, role and function in the hospital setting, to learn how concepts and research orientations of the social and behavioral sciences can be applied to the hospital and its clinical phenomena, and to become ever alert to research potentialities in these phenomena and in the many problems that face the hospital. Although supervised experience in diagnosis and therapy is provided for in this program, the experience is preceded by the plan for intimate acquaintance with the hospital. In such manner, says Professor Luchins, can the student's test reports and other communications become more meaningful and useful to hospital staff.

It is clear from the above that the practicum supervisors of students in such a program will have to be, themselves, unusually well grounded in several broad fields of psychological science and practice—especially if the teacher-supervisor wishes to implement the second major feature of Professor Luchins' program (i.e., integrating the student's current practicum experiences with his prior university courses). This synthesis is accomplished through a scheduled seminar in which, by use of both lecture and discussion, the practicum teacher attempts to integrate the students' (1) everyday experience with hospital phenomena with (2) the content of his courses in experimental and social psychology, perception, the psychology of cognitive processes, psychopathology, group dynamics, etc.

In the opinion of this reviewer, Luchins' book is an important contribution to psychology. A decade ago, Dollard and Miller ably attempted to integrate two theoretical systems; learning and Freudian personality theories. In the present book, Luchins, equally skillfully, has attempted to integrate social-experimental psychology with applied clinical practice. Thus the book will be

of value to university teachers of clinical, social, and experimental psychology, as well as to supervisors in internship centers. To the latter, the reviewer reveals his own impression that, because of the scope of Luchins' program, even the implementation of one small phase of it would be a rewarding accomplishment for any supervisor.

## The Cerebral Cortex in English

Gerhardt von Bonin (Trans.)

*Some Papers on the Cerebral Cortex.* (Trans. from the French and German.) Springfield, Ill.: Charles C Thomas, 1960. Pp. xxiv + 396. \$11.50.

Reviewed by A. EARL WALKER

who is Professor of Neurological Surgery in the Johns Hopkins University School of Medicine.

**I**N this volume the distinguished Professor of Anatomy of the College of Medicine of the University of Illinois in Chicago has brought together a selected group of classics dealing with the cerebral cortex. With one exception these have all been translated from the French or German. These anatomical and physiological milestones enable one to follow the mental processes of the biologists, who developed the present concepts of the cerebral cortex.

In his introduction Professor von Bonin gives a brief historical account of the evolution of thought regarding cortical function and concise biographical sketches of the authors whose works he has translated. These remarks provide a nice orientation for the remainder of the volume.

Papers by Flourens, Baillarger, Broca, Fritsch and Hitzig, Munk, Goltz, Meynert, Flechsig, Brodmann, von Monakow, Ramon y Cajal and Leyton, and Sherrington illustrate the transition from a holistic view of the cerebral cortex to an areal one related to localization of



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function. Professor von Bonin makes no comment regarding the views expressed by the various authors, nor does he attempt to suggest the value of their contribution to present knowledge of the cerebral cortex. Nevertheless, since some of these papers were published in not readily accessible journals, this book should be of considerable value to all interested in the cerebral cortex, especially those persons who do not have a mastery of French and German. The translations, in good English, are a faithful rendition of the originals.

Few would criticize the inclusion of

these classics in a book on the cerebral cortex, particularly as the author, with one exception, has limited his sources to papers published in other than the English language. One might, however, ask if a discussion of the evolution of knowledge of the cerebral cortex can be complete without some reference to Gall, Dax, Bouillaud, and Hughlings Jackson.

All biologists and physiological psychologists who are interested in the cerebral cortex will find this book useful. Those not well versed in French and German will thank the author for his long labor of love.

readers may wonder whether 'fundamental' ingredients do exist in *educational psychology*.

Perhaps the salient shortcoming of the volume lies in its effort to encompass a plethora of topics within the confines of one cover. Depth succumbs to breadth and results in a tendency toward superficial treatment. Yet, despite the broad coverage, significant omissions are evident. The contributions of B. F. Skinner and of E. R. Guthrie to learning theory are not mentioned. Material from the 'Dartmouth seminar' is missing, particularly Estes' analysis of 'field' theory (1954). Teaching machines and automated devices are ignored. Studies of recent vintage in problem solving, such as Buswell's 1956 monograph on thinking patterns, do not appear. Nor is psychoanalytic theory and its relationship to learning disability touched upon.

Other important topics receive but minimal treatment. For example, evaluation of pupil learning via teacher-made tests is allotted a scant two pages. Also the volume lacks critical appraisal of many of its generalizations. One author concludes: "Separation over a period of time accompanied by deprivation of needs is likely to produce an incapacity to achieve close and intimate human relationships" (p. 328). The statement has merit but fails to incorporate criticisms by Pinneau of Ribble and Spitz, or to identify institutionalized children as a major source of separation data.

Despite these limitations, the volume possesses strengths. It is well written; it covers topics of contemporary concern; and it communicates the complexities of teaching in an educational structure which values individual differences. The authors uniformly attempt to translate psychological findings into implications for school practice, and the pupil-centered theme fits particularly well with the interests of prospective teachers in elementary education. In these respects the volume has achieved a generous measure of success.



*From among the docile and humble it is possible for saints to emerge, but seldom scholars.*

—S. RAMÓN Y CAJAL

## What Is Educational Psychology?

Charles E. Skinner (Ed.)

*Educational Psychology*. (4th ed.) Englewood Cliffs, N. J.: Prentice-Hall, 1959. Pp. xii + 755. \$7.95.

Reviewed by EDMUND V. MECH

*Dr. Mech is Associate Professor of Social Work and Social Research at Bryn Mawr College and also Director of Research in the Children's Aid Society of Pennsylvania. He has a PhD in educational psychology from Indiana University and has taught educational and developmental psychology at Oklahoma, Penn State, and Johns Hopkins Universities. There must be such a subject as educational psychology if you can teach it.*

THIS volume appears during a period when psychologists are concerned with defining the domain and content of educational psychology. Few are entirely certain of its essential components. One indication of a mixed state of affairs is the continued difficulty experienced by the *Annual Review of Psychology* in defining the area and its decision to offer chapters for educational psychology only occasionally.

Twenty-two authors contributed to this textbook, which continues a series begun 25 years ago by the editor, Dr. Charles E. Skinner, Professor Emeritus at New York University and now Visit-

ing Professor at Southern Illinois University. Designed for use in teacher-training programs, this edition strives to reflect fresh content and shifting emphases in educational psychology.

Four major topical segments appear: *Personality and Adjustment, Growth and Development, Learning, and Evaluation and Measurement*. A pupil-centered approach is advocated and the interrelatedness of emotional, intellectual, physical, and sociocultural factors is emphasized. Such a multifocal treatment is quite compatible with trends during the past decade.

Knowledge of learning processes is described as the core of a teacher's professional education. Learning is not, however, viewed as the only area requiring study. Approximately two-thirds of the volume is devoted to 'other' topics, including guidance, special education, and statistics. Such diversity of content makes it difficult to separate educational psychology from selected splinters of general psychology, child development, or mental hygiene. Eclecticism characterizes this work while theoretical considerations suffer, and

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# Behavior and the Brain

Mary A. B. Brazier (Ed.)

*The Central Nervous System and Behavior.* (Transactions of the Second Conference, Princeton, N. J., 22-25 Feb. 1959.) New York: Josiah Macy, Jr. Foundation, 1959. Pp. 358. \$4.75.

Reviewed by SAMUEL SUTTON

who is Associate Scientist in Biometrics Research for the New York State Department of Mental Hygiene, and Research Associate in the Department of Psychiatry of Columbia University. He has been working for several years now on functional psychoses as related to altered brain function.

THESE proceedings, the second in a series of conferences held by the Josiah Macy Foundation on the central nervous system, testify to the present vigorous and efflorescent state of brain-behavior research. From a stage in the early 40s where there were few workers, little data, and the concept of a 'trace' was little more than an article of faith, we have moved in two decades to the point where we are almost embarrassed by riches. As the volume shows, new findings have not only outstripped our ability to synthesize but have also gone beyond the very conceptual tools with which we attempt to formulate both the problems and the results.

Many psychologists, trained in the tradition of careful sterilization of concepts and terminology before permitting them to be exposed to public view, may find somewhat disturbing the willingness of these investigators to borrow terms both from lay language and from the introspective psychology of the last century. From a presentation of data by the Russian physiologist, V. S. Rusinov of Moscow, on human recordings of evoked potentials for expected stimuli which have been withheld after training, the discussion shifts into a consideration of the adequacy of both our

language and our philosophical underpinnings for dealing with the complex problems of brain-behavior research. Then, before the reader is returned to the data level by F. G. Worden's presentation of a longitudinal study of electrophysiological correlates of learning in the cat, he finds himself exposed to the questions of the nature of scientific models, the meaning of explanation, Frederick Engels' ontology, levels of integration in the nervous system, the use of set theory and computer models, and finally the relation of the psychological to the physiological level of abstraction. In summarizing the proceedings, Robert Galambos even raises the question of the adequacy of the present distinctions between learned and unlearned behavior.

While some of this discussion may seem far afield, it is clear that investigators dealing with the relation of the nervous system to behavior are forced by their subject matter to re-examine first principles in order to break out of the conceptual straitjackets that hamper progress quite as much as do inadequate techniques.

NEVERTHELESS this concern with basic principles is only a motif which appears and reappears throughout these proceedings. The conference was organized around the presentation of several lines of recent research. More than half the report is devoted to studies of a group of brain structures which has come to be known as the limbic system, consisting of phylogenetically old cortex and related subcortical structures. J. W. Papez in the thirties drew attention to the relations of the various parts of this system to each other and suggested that they might be involved in emotional behavior.

In more recent work these structures have been ascribed a variety of complicated functions. P. D. MacLean, working primarily with stimulation techniques, considers that the limbic system is involved in a group of behaviors whose common denominator seems to be their relevance to self-preservation and species preservation. K. H. Pribram analyzes the symptoms of the amygdalectomized animal, in respect of short-term memory disturbance or, more spe-

cifically, as defects in the hierarchical and sequential ordering necessary to perform an integrated series of acts. Much of this discussion centers around the difficulty of specifying a common denominator for the complex disturbances resulting from a given lesion. Here the discussions touch most closely on problems encountered in clinical neurology and psychiatry. Despite the fact that in animal work the experimenter can control with fair precision the locus in the brain of either lesions or stimulation, the specification of the key changes in behavior, or of their common denominator, continues to present a most difficult problem.

Endre Grastyán of Hungary, using electrical recording from the hippocampus during conditioning, finds the slow wave activity of this structure to be related to the orienting of the animal to a familiar stimulus whose meaning is still uncertain, that is to say, the activity appears neither when the stimulus is completely new, nor after conditioning is completed, but only when the stimulus still has an intermediate status. M. E. Olds, working with electrical stimulation of the same structure, finds that the animal cannot form a new learning set if stimulation occurs during the early part of the learning, but can perform the task if stimulation is not introduced until learning has occurred.

Somewhat less familiar, at least to this reviewer, is Jan Bures' ingenious adaptation of the well-known depression of Leao to produce a temporary functional ablation of the cortex. In rats trained to make a conditioned avoidance response, potassium chloride is applied to a point on the cortex. The application of KCl, called reversible decortication, temporarily abolishes the performance of the CAR and also impairs the escape response to shock. The animal retains postural reflexes and presumably other unlearned behavior. Recovery of the CAR is shown to parallel the EEG changes characteristic of recovery from spreading depression.

This volume is a verbatim record of discussion among investigators actively involved in the field. The conference included members from the United States, Holland, Russia, Czechoslovakia, and



Hungary. One aspect of the conference which will be equally interesting to the specialist and nonspecialist is the fact that scientists from capitalist and socialist countries are so closely related in their interests and findings, and apparently have much to give each other.

## Adjusting Freshmen

Fred McKinney

*Psychology of Personal Adjustment: Students' Introduction to Mental Hygiene.* (3rd ed.) New York: John Wiley & Sons, 1960. Pp. xiv + 490. \$6.50.

Reviewed by H. MELTZER

who is Vice-President and Director of Human Relations of the Archard Paper Company of St. Louis, President of the Human Relations Research Foundation in St. Louis, and Lecturer in the Washington University Graduate School, where he gives a course in Human Factors in Industrial Management. He was one of the pioneers of the orthopsychiatric group and is a past vice-president of the American Orthopsychiatric Association.

PROBLEMS of adjustment college students have always had. But it was not until after the mental hygiene movement got under way that Dr. McKinney, Professor of Psychology at the University of Missouri, amongst others, saw the need for a book on psychology of personal adjustment for freshmen. This was over twenty years ago and the book was well enough received to warrant a revision in 1949, which in turn, being even better received, now warrants the present third revision.

As in the previous editions, the material presented contains chapters on many areas of adjustment, including creative adjustment, and also supplementary chapters giving background and knowledge for understanding the problems of adjustment. As in the previous editions, too, the book contains many

lists to serve as maxims for living. To the last list of sixteen precepts of mental hygiene, one more is now added: "The need for perspective."

Much knowledge of personality dynamics that bears on mental hygiene has been amassed in this period. It is, therefore, not surprising that the theme, set by the author for the third edition, has changed from satisfying the need for "a basic psychological text which frankly attacks problems of the students" to being "an attempt to enable the reader to deal with his conflicts, frustrations, and emotional states in a manner that reduces the anxiety, tension, self-centeredness, and aimless defensive activity and redirects his activity toward a perception of the cause of his condition and a discovery of the activities that may possibly remove it and satisfy his needs." Does this modified form of expressing the theme of the book characterize the changes made and the extent of them for meeting the new needs set by the author?

THE first impression one gets of the revised edition as compared to the previous one is that it is much smaller, 490 pages against 752. It contains the same number of chapters, sixteen, and only some of the earlier ones are recaptioned and expressed in somewhat more general terms for reorganizing the content. At the end of each chapter is found a short list of current supplementary readings, all dated 1950 onward. There are references to references but no listing of those references because of one completely new feature, namely, the references, along with merit rating and preinterview sheets, are included in a companion volume called *Teaching Personal Adjustment*.

A closer look indicates that the decreased number of pages is not indicative of as much reshaping as first appears. Not only are the references included in the companion volume, but the students who are resourceful enough to want to follow up on some of the 784 references mentioned will necessarily have to buy the companion volume which is intended for use in teaching. Whether this is a marked advantage is questionable unless two things are kept in mind. If freshmen college

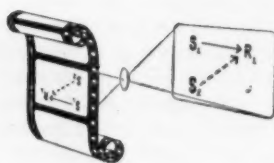
students are, as some people accuse them of being, cultural infants, Dr. McKinney accepts the fact that infants are what they are and that they need that kind of feeding. He also admits that some of the people who teach the freshman course are not well enough trained to go on their own without help in the form of such teaching material. If such is the case, the present arrangement certainly promises to satisfy the teacher's and the learner's needs better than previous issues. The judgment of the publisher and author on that ground is probably correct. Unfortunately this reviewer does not have a copy of *Teaching Personal Adjustment*.

Considerable attention is given in the text to the problems of values and group dynamics but the material as presented is more likely to stimulate interest than to help attain a direction for a set of human values. For example, we are advised that some leaders bring prestige to their offices. Then there comes a list that includes Eleanor Roosevelt, F. D. R., Mussolini, Gandhi, Lawrence of Arabia, and Huey Long. By contrast, Fritz Redl in his book, *Mental Hygiene and Teaching*, selects Gandhi and Socrates as leaders, not because they were well adjusted but because they represented people with a sense of values whose contribution was in line with human progress and had human values as a reference point. Though McKinney has a sentence which says that adjustment does not necessarily mean conforming, and puts in his list on aims of education an item which favors critical and constructive thinking, there is nothing in his book which encourages anything more than tolerance, nothing about the application of critical intelligence. The independent and self-assertive literary people who favor a spirit of independence and self-assertiveness will interpret this book as representing psychiatric baby-sitting in the name of mental hygiene.

For its purpose—to help college freshmen find adjustment—in view of the nature of students going to college and of the teachers available to teach them, this book, even more than the previous editions, promises, with the help of the teaching aid, to be even more successful than the earlier editions.

# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## EDUCATIONAL TV: AMBIVALENCE AND OPTIMISM

*Teaching by Television.* A Report from the Ford Foundation and The Fund for the Advancement of Education. The Ford Foundation, 477 Madison Avenue, New York 22, N. Y.

Alexander J. Stoddard

*Schools for Tomorrow: An Educator's Blueprint.* The Fund for the Advancement of Education, 477 Madison Avenue, New York 22, N. Y.

J. Lloyd Trump

*Images of the Future: A New Approach to the Secondary School.* The Ford Foundation, 477 Madison Avenue, New York 22, N. Y.

Reviewed by FRED MCKINNEY

Few psychologists should be able to speak more authoritatively about the problems and potentialities of teaching by television than Professor McKinney of the University of Missouri, whose impressive experience with this medium includes being producer or principal in 13 kinescoped programs for the National Educational Television and Radio Center (Not in Our Stars) and in 13 other kinescopes produced by Stephens College. He has taught several full semester courses in psychology and other subjects over television and has broadcast a weekly 15-minute educational program over Station KOMU for the past three years. During 1958-1959 he was president of APA's Division 2, while visiting at the University of Ankara and traveling in the Middle East.

DESPITE the over-all contribution reported in these booklets—which represent imaginative and drastic educational innovations, and rapid achieve-

ments in the use and understanding of TV in teaching—and notwithstanding a strong personal bias in favor of educational TV from several years of varied experience with it, I felt a vague dissatisfaction as I read them. I suspect my feelings will be shared by many of my colleagues. Teachers more than other groups are dragging their feet on 'instructional' TV. At the Pennsylvania State University, where educational TV is well established, the attitudes of the faculty have been described as follows: "A few are quite negative toward it, some are enthusiastic and the largest proportion appear to be undecided and indifferent. Many faculty members have not observed television classes" (*Teaching by Television*, p. 27).

The basis for my uneasiness, as I perused these booklets, is suggested in the subtitle of one of them: *An Educator's Blueprint*. In this publication, as well as in *Images of the Future*, a clear-cut primacy was given to the structure of education, whereas it seems that educational functions and the interactions between two motivated learners should have at least equal stress. Both booklets mention individual differences in interests, quality of education, and importance of individual problem solving. However, when contrasted with another recent publication, an ACE conference report, *College Teaching by Television* (ed. J. C. Adams, C. R. Carpenter, & D. R. Smith, Amer. Council Educ., 1958), one sees the submerged element. There is not the same detailed and pervasive emphasis in these reports on the dynamics of the learner, his motivation, the teacher-student interaction, and the student 'reward,' nor is there the wholesome criticality which is usually evident when teachers and research workers are added to the administrative team in the planning phases of edu-

cation. Terms like *limitations*, *resistances*, *need for evidence*, and *criteria*, found throughout the ACE report, are minimal in these booklets.

The essentials of the educational process doubtless were assumed in the reports, but the absence of clear and detailed statements of their implications is a hazard that represents one of the major deficiencies in some of today's ETV. In our emphasis on *presentation* in 'instructional' television we may lose sight (as I felt at times I did) of the importance of *interaction with the student* in stimulating him to learn, solve problems, and occasionally gain insights and new perspectives. Artful manipulation of the student's environment has and can arouse insights and initiate inner growth; it can also become a hazard to the effective utilization of individual motivations.

The suggestions made in *Images of the Future* and *Schools for Tomorrow* that good teachers be multiplied and top-quality people be attracted into teaching are excellent—but neither is specifically detailed. The current difficulties in schools are seen mainly as a serious teacher shortage with greater numbers to be educated in a more complex world. The important *flexibility* emphasized in the report is hopeful. It is suggested that the use of school time and space is to be radically altered to free the teacher for what he can best do and to challenge the student more fully.

MORE dynamic is *Teaching by Television*, reporting results of research and line operations. In approximately 60 pages it presents a concise review of three years of experiences and experiments in the use of TV on multiplying the effectiveness of able teachers. At all levels and with all kinds of education, students learn as much as at present, and in some cases significantly more, from televised instruction as from conventional teaching.

The Washington County, Maryland, program is a well-planned experiment in the use of classroom TV in a school district. It was launched in the fall of 1956 and has used, in addition to large classes, smaller sessions under classroom teachers who were alert to the scholar's

difficulties and who supervised drill and problem-solving sessions. Significantly, the year was preceded by a summer workshop for teachers at which the learning process was analyzed to ascertain which functions could be *performed best by studio teachers and which best by teachers in classrooms*. Again one notes an emphasis on teachers without equal discussion of the learner.

Another project (The National Program in the Use of TV in the Public Schools) involved about 40,000 students and over 200 representative elementary and secondary schools. The details of courses were planned in a workshop. The students were prepared by the classroom teacher for the TV presentation, and a discussion followed it. This project resulted in "rethinking of the curriculum," demonstrating "superior teaching" to teachers in the program, broadening instruction to nonschool audiences, and wider use of the library. Happily there was reported a saving in the teacher's time for individualized instruction, and a reduction of tardiness and absence. Of the 110 comparisons of measures of student achievement and attitude, 68 favored the TV and 29 favored the control classes.

On the college level a wide range of courses have been taught by TV—from General Chemistry to Creative Art. It has been found that more material can be covered on TV and that televised sessions can be combined effectively with face-to-face sessions. Polls show college students usually prefer a conventional class and mention contact with the instructor as one important factor in their choice. Those who do prefer TV appreciate the careful selection of teachers and the extra preparation demanded for the medium. It is impressive how much data have been collected in these experiments, including the effect of teaching without a text, the use of special tests, the effects of the course on authoritarianism and on the continued study of psychology. The evaluative emphasis, flexible attitude, deliberativeness with which many of the problems reported are attacked, and the conciseness of the report, make this volume a valuable one which encourages optimism about intelligent exploration in ETV.

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# ON THE OTHER HAND



## KURIE AND THE CREATIVE PROCESS

May I have the privilege of replying to a few of the main points in the recent review by Professor Frank Barron of my book, *The Neurotic Distortion of the Creative Process* (CP, May 1960, 5, 170f.)?

(1) Professor Barron states that my point of reference is the accepted Freudian theory of thinking. In this he is mistaken. Actually the theory of thinking which I present in this book involves basic departures from the currently accepted version of Freud, such that many of my colleagues are quite disturbed about it.

(2) Barron attributes to me the idea that "the neurotic process and the creative process are opposite to one another in their essential nature." Quite to the contrary, my thesis is that the neurotic and the creative processes use precisely the same preconscious mechanisms, but that these preconscious processes and their conscious symbolic representation in creative work can be blocked, distorted, and reduced to sterile stereotypes by the play of unconscious neurotogenic conflicts and forces.

(3) Barron also attributes to me the conviction that "nothing creative can come from the neurosis." My argument is rather that wherever neurotogenic unconscious processes exercise a dominant influence they restrict, imprison, and render stereotyped and repetitive far more than they create.

(4) Further, he attributes to me the idea that conscious processes serve no creative function at all. It is true that I emphasize certain restricting and pedestrian aspects of conscious processes, but I also point out that they perform several vital functions in relation to creativity—for example, to sample the creative preconscious stream, to ruminate about it for retrospective and retroactive self-criticism (communication with oneself), and to communicate the results to others.

(5) He also finds me making certain large predictions about the future of creativity. What I actually argue is that it is important for the further evolution of human culture to find out what qualitative

and quantitative changes in creativity would result from a reduction of the role played by neurotogenic forces in the creative process. It is in this context that I protest against the biases which block investigations along these lines.

(6) In developing his own point of view, Barron makes certain assumptions about creative artists: the artist has something called "unusual sensitivity" or "exceptional symbolic scope." Unfortunately criteria for such claims and data to support them are lacking.

(7) Finally, I must remind CP's readers that I pointed out repeatedly the difference between the use of psychotherapeutic techniques for therapeutic purposes, and the use of techniques derived from psychotherapy for the prevention of neurosis. I likened this to the difference between the prevention of fission and the re-fusing of that which has already been subjected to fission. My argument is that throughout the educational process educators must learn how to apply techniques borrowed from psychotherapy to prevent neurotic distortion of the educational process itself. This is quite different from imposing on educators the massive burden of healing those who are already ill.

These corrections should clarify my position. When Dr. Barron thought he was disagreeing with me, he was disagreeing with points of view which I cannot find in my volume.

LAWRENCE S. KURIE  
The Sheppard and Enoch Pratt Hospital  
Towson, Maryland

## RITCHIE ON MADSEN

Benbow Ritchie's review of K. B. Madsen's *Theories of Motivation* (CP, July 1960, 5, 228-229) is 'confusing. It seems that, because of the reviewer's adherence to a particular view of the scope of the study of motivation ("how states and dispositions influence behavior"), he does not address himself to the aims of the book—"A comparative study of modern theories of motivation." The review fails to bring out the positive values in such a study, because of the restrictiveness of the implicit definition of motivation.

One function of a theory in psychology is to make clear the assumptions within which we are working and to raise new questions which will require new approaches for their answering. Even though Madsen's book stops disappointingly short, a thoughtful look at some of these theories should be stimulating to the growing efforts to break out of the usual ways of framing the problems of 'motivation.'

ANN D. SALOMON  
Massachusetts Mental Health Center

## BRAIN DAMAGE AND PERSONALITY

Dr. B. B. Wolman's review of Silvano Arieti's *American Handbook of Psychiatry* (Basic Books, 1959; CP, June 1960, 5, 177-179) I found considered and comprehensive. On page 178 there is, however, a grievous misquote which practically reverses the meaning of Brosin's discussion of the relationship between personality factors and brain-damage. The quotation should read (Arieti, *op. cit.*, II, 1190): "Ruesch, Harris and Bowman in their intensive studies also found that in a large proportion of cases the brain-damage is of secondary importance, while the pre-traumatic personality is of primary importance in determining the nature of the post-traumatic state."

OSCAR A. PARSONS  
Medical Center  
University of Oklahoma



Nothing is judged more carelessly than people's characters, and yet there is nothing about which we should be more cautious. Nowhere do we wait less patiently for the sum total which actually is the character. I have always found that the so-called bad people gain when we get to know them more closely, and the good ones lose.

—GEORG CHRISTOPH LICHTENBERG



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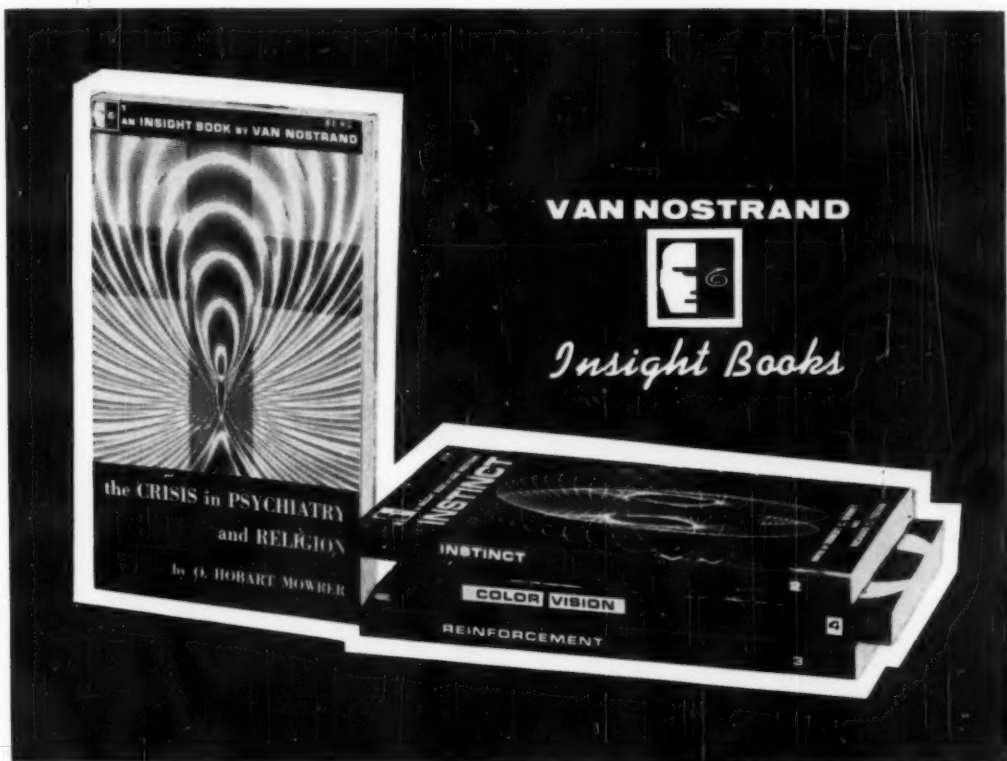
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